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The William Harvey Issue

IN June three hundred years ago William Harvey, the English physician who established 'modern' physiology by his masterfully conceived experiments that led to the discovery of the circulation of the blood, died in London where he had lived and practised the greater part of his life. Earlier in 1657 (February) there had occurred the death of Jean Riolan, his most eminent opponent to the theory of the circulation. But Harvey's brilliant discovery has endured through the centuries as probably the greatest single advance in scientific knowledge until the work of Pasteur and Lister.

In this tercentenary year, tributes to the memory of William Harvey are taking many forms. Exhibits of his works will be found in those libraries all over the world fortunate enough to possess copies. An international congress will be held in London June 3d to 8th ending in a visit to Folkstone where Harvey was born. Professor Kenneth J. Franklin has prepared a scholarly yet lively new translation of *De motu cordis* under the auspices of the Royal College of Physicians which has been published by B. H. Blackwell, Ltd. And there will be many other tributes.

The Editors of this *Journal,* wishing to mark the occasion in some way, invited scholars from many countries to take part in a special issue. Translators whose native tongue was the language of the original paper have rendered into English those submitted in Swedish, German, French, Spanish, and Italian, an effort being made to maintain a nice balance between the flavor of the original and idiomatic English.

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The Germ of An Idea
or
What Put Harvey on the Scent?

THE LORD COHEN OF BIRKENHEAD*

FEW studies are less rewarding than those which attempt retrospectively to understand the workings of the mind of a genius. Yet I propose quite briefly to suggest an answer to the question, What led Harvey not simply to assert that the blood circulates, but to *demonstrate* beyond all reasonable doubt that the blood circulates?

Harvey was not the first to criticise the Galenic doctrine. Leonardo da Vinci had found it impossible to pump air directly into the left heart from the trachea and bronchi, though this transit was essential to the Galenic view, which also demanded a perforated ventricular septum so that the 'natural spirit' might flow from the right to the left side of the heart. But Vesalius failed to find these perforations. In the first edition of *De humani corporis fabrica* (1543) he wrote:

> The septum of the ventricle, composed as I have said of the thickest substance of the heart, abounds on both sides with little pits impressed in it. Of these pits, none, so far at least as can be perceived by the senses, penetrates through from the right into the left ventricle, so that we are driven to wonder at the handiwork of the Almighty, by means of which the blood sweats from the right into the left ventricle through passages which escape human vision.

He was later to confess that he "accommodated his statements to the dogmas of Galen not because he thought that these were in all consonant with truth but because in such a new great work he hesitated to lay down his own opinions, and did not dare to swerve a nail's breadth from the doctrines of the Prince of Medicine."

Michael Servetus in his *Christianismi Restitutio* (1553) had postulated a lesser (pulmonary) circulation and not a perforated ventricular septum as the mechanism of aeration of the blood, and had based this on morphological grounds:

> This view is confirmed by the conspicuous size of the artery-like vein which would not have been made so large and so stout, and would not discharge from the heart itself such a power of very pure blood into the lungs,

* The University of Liverpool, England.

for the mere purpose of nourishing these organs. Nor would the heart serve the lungs in this manner, especially since at an earlier date in the embryo on account of the little membranes of the heart, the lungs themselves are up to the hour of birth nourished from other sources, as Galen teaches.

This view was also expressed by Matteo Realdo Colombo, who succeeded Vesalius at Padua in 1544; but he was of doubtful integrity and might have seen a manuscript of Servetus' *Restitutio* and claimed the discovery as his own. Yet for the frank indictment of his fellow anatomists in his *De re anatomica libri XV* published posthumously in 1559, he merits our favour: "But for these it is enough that Galen said so. What? To think that some folk in our time swear to the dogmas of Galen about anatomy so that they dare to assert that Galen ought to be taken as gospel, and that there is nothing in his writings which is not true!"

But nearest to Harveian views came Andreao Caesalpino, professor of medicine at Pisa. In his *Quaestiones peripateticae* (1571) he infers from the directional closing and opening of the heart valves that the heart in systole propels blood into the aorta and pulmonary artery, and in diastole receives blood from the venae cavae and pulmonary vein; and in his *Quaestiones medicae* (1593), there is a hint that he conceived blood to flow from arteries to veins and along the veins to the heart. He noted that veins when ligated swell *below* the ligature and recognised that this was in opposition to Galenic teaching.

There is no doubt that Harvey knew of these authors' writings. He quotes a few, including Colombo, but in the dedication to *De motu cordis* he expressly declares that he had "no purpose to swell this treatise into a large volume by quoting the names and writings of anatomists, or to make a parade of the strength of my memory, the extent of my reading, and the amount of my pains."

Yet it is not to his reading but to his sojourn in Padua and probably to a chance observation that we owe *De motu cordis*.

The University of Padua prepared the soil. Of all the universities of Europe, Padua was the least bound by authority. In 1404 it had come under the rule of Venice, which at that time and for long after was the most anticlerical state in Europe. Padua fostered the spirit of independent enquiry and condemned the unquestioning acceptance of authority, so that its teachers did not seek for 'final causes' or explanations of the 'why?' of natural phenomena in theological and supernatural terms. Rather did they pose the question, 'how?' and sought to answer this by experiment and quantita-

tive methods. Had not Padua given shelter to Copernicus and Galileo? So we are not surprised that Harvey went straight to the 'fabric of nature' and to 'dissections' rather than books, and that he looked upon the heart as "a piece of machinery in which, though one wheel gives movement to another, yet all the wheels seem to move simultaneously."

But is there in Harvey's writing any hint of how he was led to formulate his hypothesis of circular motion? Osler quotes Boyle as saying that in the only conversation he ever had with him Harvey acknowledged that a study of the valves of the veins led him to the discovery of the circulation of the blood. This is readily understandable for it was Hieronymus Fabricius ab Aquapendente, Harvey's teacher at Padua, who in his *De venarum ostiolis* (1603) had shown the direction of the orifices of the valves of the veins, and the effects of ligatures, though Fabricius interpreted these in terms of Galenic doctrine, namely, that the valves slowed the centrifugal flow of blood and checked its accumulation in large quantities at the periphery.

Despite Harvey's assertion, I suspect that a chance (though inaccurate) observation led Harvey to postulate a continuity of artery and vein.

In his Introduction to *De motu cordis,* Harvey recalls that Galen had demonstrated the error of Erasistratus, who maintained that arteries were filled with air, by carrying out the simple experiment of including a part of an artery between two ligatures, and slitting it open lengthways. "Nihil praeter sanguinem reperies" (you will find nothing but blood) wrote Galen; and, as Harvey comments, "thus he proves that the arteries contain blood only." Then Harvey continues, and I quote the original to avoid the chance of mistranslation:

> Unde nobis ratiocinari licet : si eundem sanguinem, qui venis similater ligatis, et rescissis inest, inveneris in arteriis (quem-admodum in mortuis et aliis animalibus saepius ego expertus sum) eadem ratione concludere nos possumus, arterias eundem sanguinem, quem venae, et nihil praeter eundem sanguinem continere.
>
> We too may be allowed to reason similarly : if we find the same blood in the arteries and veins, having ligatured them in the same way (as I have myself repeatedly shown both in the corpse and in living animals) we may rightly conclude that the arteries contain the same blood as the veins, and nothing but the same blood.

I suggest that this was the spark which lit Harvey's imagination. If the blood in veins and arteries is the *same* blood, then the Galenic view is untenable and there must be continuity or com-

munication between arteries and veins. And this belief was supported by the overt inconsistencies in earlier hypotheses. The introduction to *De motu cordis* is, in fact, a penetrating critical analysis of past theories.

Having formulated his hypothesis, Harvey then applied the scientific method which he had learnt at Padua. His first problem was to find the animals best suited to his purpose of "discovering the motions and uses of the heart." But in warm-blooded animals he could "neither rightly perceive at first when the systole and when the diastole took place, nor when and where dilatation and contraction occurred, by reason of the rapidity of the motion, which in many animals is accomplished in the twinkling of an eye, coming and going like a flash of lightning." The movements had to be seen in slower motion and Harvey found his material in the slowly beating heart of cold-blooded animals and the dying, flagging heart of the warm-blooded animal.

Then came his accurate observations and records of what occurs to the heart and blood vessels in the cardiac cycle. And to test his hypothesis he collected facts from many sources—human and comparative anatomy, embryology, animal experiment, and clinical observations—and these are set out with such clarity and his reasoning from them is so cogent that *De motu cordis* might well serve as a well-nigh unimpeachable example of scientific method.

And he brings to his support for the first time (as Singer has emphasised) *quantitative* considerations in the method he had seen followed at Padua. He measured the mass of blood which passed through the heart in a given time and showed that in the course of half an hour the heart would have to propel a larger quantity of blood than is found in the whole body; hence it could be explained only by a system of circulation.

What is surprising is that Harvey did not himself perform a simple yet crucial *perfusion* experiment to show that the passages from arteries to veins are direct and continuous. He knew the method of perfusion and had used it to show that water injected into the pulmonary artery passes into the left ventricle (see his letter to Paul Marquand Slegel dated the 26th of March 1651). But this does not detract from Harvey's achievement. For this was not solely an epoch-making contribution to knowledge, but a challenge to man as a reasonable being to free himself from the shackles of authority and to pursue the truth wherever it may lead.

Harvey's Animals

F. J. COLE*

THE present paper is based on the following works by Harvey: (i) The autograph MS. of the three readings or lectures delivered on 16, 17, 18 April 1616 before the College of Physicians at the age of 37, but not published until 1886;[1] (ii) his work on the circulation published in 1628 and on the two letters which Harvey addressed to Riolan in 1649; (iii) his work on generation completed in 1651. The first of these had a somewhat interesting history. The manuscript, which had been acquired by Sir Hans Sloane, was bequeathed to the British Museum in 1753. Some years later it had disappeared, and in spite of several careful searches had not been recovered, the chief librarian concluding that it had been borrowed and had gone the way of borrowed books in general. However, over one hundred years later, in 1876, it was discovered amongst some duplicate *printed* books, and restored to its place in the Manuscript Department. This certainly explained the temporary loss of the MS., but in a way that made it much more alarming, and hence the College of Physicians decided that it must be transcribed and published in facsimile. This was done, and the transcription of Mr. Edward Scott of the Manuscript Department of the British Museum is literally beyond praise. An English translation with commentary is desirable.

These three lectures to some extent advise us of Harvey's anatomical interests, but first and last they are notes jotted down to remind the lecturer of his obligations to the audience. He mentions 128 types of animal life which fall into the following categories: Zoophyta, Worms (3),[2] Crustacea (4), Insecta (12), Arachnida, Mollusca (6), Fishes (18), Amphibia (3), Reptilia (4), Birds (33), Mammals (43), total 128. The species in question are: Zoophytes; worms unspecified 4,[3] Earthworm, Leech; Crustacea unspecified 3, Crab 3, Lobster 3, "Shrimp"; Insects and Arachnids unspecified, Ant, Bee 2, Butterfly 4, Caterpillar, Flies 2, Cranefly, Pediculus 2, Pulex, Silkworm, Spiders 4, Wasp 2, Weevil; Mollus-

* Kingwood, Henley-on-Thames, England.

1 These were Harvey's first anatomical lectures under the Lumleian Trust.

2 The figures in parentheses refer to the number of species mentioned. When no figure is given the species appears only once.

3 The figures without parentheses refer to the number of times a species is mentioned.

ca, Cockles, Mussels, Mytilus, Oysters 2, Slugs, Snails 3; Fishes unspecified 32, Barbel 5, Conger, Dogfish, Eel 4, Flounder 2, Herring, Lamprey, Lophius 2, Mackerel, Plaice 4, Rana marina-Lophius, Rhombus, Roach, Smelt, Sole, Sting Ray 9, Torpedo 2, Whiting; Amphibia, Frog 13, Eft and Newt 3, Toad 3; Reptilia, Lizard 2, Serpents 16, Testudo 12, Viper; Birds unspecified 32, Blackbird, Cockatoo, Cormorant, Crane 3, Crow, Duck 9, Falcon and Sparrow Hawk 6, Fowl 6, Goose 18, Hedge Sparrow, Kite 2, Lapwing, Ostrich 3, Owl, Parrot, Partridge, Pheasant, Pigeon 9, Puffin, Quail 2, Ruff Bird 2, Sparrow 2, Starling, Stork, Swallow, Thrush 2, Turkey, Woodcock 3, Woodpecker 2, Yellowhammer, Turtle Dove; Mammals, Apes 8, Ass 2, Badger, Balaena, Bear, Camel 3, Cat 3, Cetacea, compound stomach (see Ruminantia), Chamois, Deer 5, Dog 25, Dolphin, Elephant 8, Goat Ram 5, Grampus 2, Greyhound 2, Guinea Pig 5, Hedgehog 2, Horse 24, Mice and Rats 17, Mole 3, Monkeys (see Apes), Musk Deer, Homo 2, Orca, Ox 11, Phoca 7, Pig 6, Polecat, Porcupine, ?Porpoise 6, Quadrupeds oviparous and viviparous 4, Rabbit and Hare 14, Ruminantia 2, Sheep 10, Stag, Stoat 2, Weasel 5, Whales 4, Wolf.

This is a long and formidable list, but it is perhaps safe to assume that the types mentioned only once or a very few times are based on Harvey's study of the literature. Judging from the use he made of them we may say that Harvey was not specially interested in the invertebrates, but from fishes onwards he begins to display a more scientific concern in the significance of comparative anatomical detail. He can write about 64 fishes and birds without attempting to identify them, or even advising us how many specimens of each species were examined. In some cases he claims the results as his own and in others, apart from these, where the references are very numerous, it seems probable that Harvey must be describing what he has seen himself. There is no evidence that he used a compound microscope, but later Leeuwenhoek demonstrated that so much could be studied with a single biconvex lens as to encourage the belief that it was possible to watch the circulation of the blood in a small crustacean when only a single magnifying glass was available.

In the limited space assigned to this article it is possible to catalogue only a very few of the subjects dealt with by Harvey—and this I do in brief descriptive phrases. The earthworm, he says, has no heart and a crab no head. Metamorphosis in Lepidoptera. Respiration in insects. Generation in a large number of animals.

Also gut and digestion. In fishes, gut, biliary apparatus, motionless ribs, and no sternum, action of heart, respiration, brain, and auditory organ, crawling of eels after removal of heart, lure of Lophius, shape of body and its effect on internal organs, anatomical and physiological notes on Sting ray. In amphibia, experiments on the function of the chest. Jumping of frogs after removal of heart. Ligation experiments on the circulation and respiration. Brains of Amphibia and birds compared. In Reptilia, the elongated bodies of serpents, eels, and stoat compared. Respiration in winter and summer. Heart and lungs of tortoise. In birds, gut and digestion, generation, absence of diaphragm, lungs and respiration, brain, wide gullet of cormorant, and anatomical and physiological notes on duck, goose, and pigeon. In birds generally, the digestive organs are more often examined than other organs. In mammals, anatomy and physiology of gut, digestion, and renal organs of dog, worm parasites. Generation and genitalia. Thorax, thymus, and diaphragm. Respiration, heart and lungs. Walking of dog after removal of heart. Proboscis as the hand of the elephant. Gut, digestion, and fat problem. Bezoar stone in liver of goat. Horse, many anatomical and physiological notes, especially on gut and digestion, spleen, heart and lungs, respiration, and physiognomy. Mice and rats. Homo. Respiration affected by "young girls lacing." Gut, liver, and kidney. Generation and genitalia, prostate, brain. Mole. No caecum. Harvey does not know the use of the muzzle. Musk deer. Harvey's statement that the animal "respirat per caudam" must not be translated literally. Ox. Heart, lungs, and brain. Pig and boar. Adiposity. Os cordis. Thymus. Quadrupeds oviparous and viviparous. Skin and its functions. Gut and derivative glands. Heart and vessels. Rabbit and hare. Anatomical notes including heart and brain. Ruminants. Compound stomach, reticulatum, omasum, and abomasum. Sheep. Anatomical notes on skin and adiposity. "Rotten sheep." Genitalia, brain. Whales, anatomical notes and respiration.

In the first edition of the work on the circulation in 1628 and in the two controversial letters written to Riolan in 1649 the number of animals concerned drops from 128 to 49. This may be explained by the fact that the 1616 lectures consist largely of hastily written jottings intended to remind the lecturer of the magnitude and nature of his discourse. On the other hand, the work on the circulation is a purposeful disquisition which should not be weak or stray from the point. Few new types are employed, but there is

a remarkable drop in the number of the mammalian experiments which may mean only that Harvey considered that the lower vertebrates served his purpose better than the higher ones, as of course they did. He includes only 49 types of animal life in the 1628 work which were taken from the following groups: Zoophyta, Porifera, Vermes, Crustacea (3), Insecta (5), Mollusca (6), Pisces (5), Amphibia (2), Reptilia (4), Aves (10), Mammalia (11). The species used and the number of times they appear in the text are: Vermes Earthworm; Crustacea, crabs, crayfish and shrimp 8; Insecta unspecified 3, Bees, wasps, and hornets, flies, pediculi 10; Mollusca, snails, whelks, slugs, bivalves, oysters, and mussels 10; Pisces unspecified 16, eels 2, carp, barbel, tench 4; Amphibia, frogs and toads 10; Reptilia, lizard, tortoise, serpent 15; Aves unspecified 3, Fowl, chick 7, pigeon, swallow, partridge, goose 4, snipe (? woodcock), swan 3; Mammalia, seal, Cetacea, dolphin, whales 4, rabbit, mouse 4, sheep, deer, ox 6, pig, dog 4.

Here are some of the problems raised and studied in the 1628 and 1649 works.[4] Sponges. The whole body expands and contracts and is used as a heart. Earthworms have no heart—not necessary in animals which arise by spontaneous generation. In Crustacea the motion of the heart can be observed in crabs, lobsters, and shrimps, which Harvey claims to have studied with a magnifying glass and shown to his friends. "A small shrimp" found in the sea and the Thames was so transparent that the action of the heart could be easily seen as through a window. Dr. Olive Tattersall suggests that a species of Mysid such as *Praunus* could be Harvey's "shrimp." It is about 25 mm. long and quite transparent, and is admirably adapted for the study of the heart and circulation. A larger possibility would be a Gammarid in which I have myself observed the complete circulation of the colourless vascular fluid in the living animal by transmitted light. In some insects when hibernating the motion of the heart may cease according to Harvey. He had noticed that insects may have an elongated dorsal pulsating heart. He had also observed that the passage of the food down the gut could be followed with a magnifying glass in a "louse." The motions of the heart are more obvious in the colder animals such as snails, slugs, and bivalves. They are more conspicuous in larger animals with red blood than in the smaller species where the blood is colourless.

[4] Harvey's use of the following terms: Astacus is the crab, not the lobster. G [C]ammarus is the crayfish, but sometimes the crab or lobster. Crustata is our Crustacea more or less. Squilla is a shrimp. Harvey said that in English the smallest Squilla is called a shrimp.

In the latter types the pulsation can be examined with a magnifying glass, but is sluggish and may even disappear in the winter. He is of course wrong in stating that oysters and mussels have no heart, which indeed is not so easy to find in those species. In the fishes the heart has no right ventricle and auricle, which cannot exist apart from the lungs. In his remarks on the vascular system of insects and fishes Harvey refers frequently to his experiments on animals and complains that he was accused of a vainglorious love of vivisections. Where a definite heart with a ventricle is present in the Mollusca, there is always an auricle, but the heart can also be a simple pulsating vesicle. The return of the venous blood to the heart can be demonstrated by ligatures. The heart of the eel beats after removal from the body and even when cut into pieces it continues to relax and contract. Harvey is wrong in stating that there is only one auricle in the Amphibia and serpents, but is very emphatic in his belief that the venous blood is transferred to the arteries by the action of the heart in the Amphibia.

Of the reptiles Harvey believes that they conform generally to his view that although animals with lungs should have two auricles and ventricles, some pulmonate species such as serpents, lizards, and tortoises have only a single ventricle and auricle. This is not correct, since all these types have two auricles and a partly divided ventricle.

In birds Harvey is again at fault in stating that the goose and swan have no ventricles in the brain. Also he had not examined very carefully the peculiar character of the cardiac valves in birds. He describes, however, the first appearance of a pulsating red heart vesicle in the chick and makes experiments on the vitality of the heart in the pigeon. On the other hand, he failed to notice that the passage of the blood into the pulmonary and systemic vessels in birds generally was the same as in the mature mammal.

In the mammal Harvey was interested in respiration of aquatic species but did not succeed in understanding it. Against Botallus he holds that there is no passage for blood from the vena cava to reach the left ventricle of the heart in the mouse. In the sheep and dog the heart injects into the aorta about 3½ pounds of blood in 30 minutes, which is not an overestimate. The direction of the flow of blood in the internal jugular vein of the living deer is demonstrated by experiment. In the ox, dog, and other mammals the position and orientation of the valves of the veins are not invariably determined by gravity. Harvey studied experimentally motions of

the heart in cold- and warm-blooded species. Air injected into the lungs of a living dog never reaches the pulmonary vein and the left ventricle of the heart.

The number of species used by Harvey in his work on generation published in 1651 rises to 71, which may be explained by the fact that he was very interested in the problem of generation from the earliest days of his researches until the time of his death. This material was selected from the following groups: Zoophyta; Porifera; Vermes including earthworm; Crustacea unspecified (2), crab and crayfish (3); Insecta, beetle, silkworm, ants, caterpillar, grasshopper, "fly," moth, butterfly, wasp, hornet; Arachnida, spider (3); Mollusca unspecified (5), Sepia and Loligo (2); Pisces unspecified (10), skate, dogfish, sting ray, shark, cartilaginous fishes, carp, herring, smelt, mullet; Amphibia, frog 4, toad, tadpole; Reptilia unspecified, serpent 4, tortoise 2, crocodile, lizard, viper; Aves unspecified 3, air sacs (2), turkey, fowl 3, pigeon and ostrich 2, Bursa Fabricii and cloaca of fowl 2, emu, duck, pheasant, partridge, crow; Mammalia. Similarity of embryos of man, dog, horse, deer, and ox. Bitch of wolf, mouse 3, stag, horse 3, ass, ox, mole 2, dog, cat, rabbit, man, sheep, goat, swine, elephant, placenta in various Mammals.

The following topics are amongst those dealt with in Harvey's work on generation. He believed that in the simplest forms of life, such as the Zoophytes and sponges, there exists what he calls a *natural* sense, i.e., that which has no dependence on a common sensorium or brain. His famous aphorism *ex ovo omnia*, he tells us, resulted from his study of the origin of the chick from the hen's egg. In the Insecta he is specially interested in oviposition, status of the larva, and metamorphosis. Caterpillars he believes have no brain. Generation of grasshoppers. Larvae of a fly, moth, and butterfly are "imperfect eggs." Emergence of the insect from its chrysalis. How wasps and hornets are hatched. Arachnida. Oviposition in spiders. Mollusca. Nature of ova. How Sepia and Loligo develop.

Many fishes develop externally and the eggs become fertile in the water but are only imperfect when shed. Harvey, however, was stating a partial truth when he claimed that all the eggs in the ovary are of one size and what he calls "imperfect." He was familiar with the abdominal swim bladder of fishes discovered earlier by Rondelet and made observations on various cartilaginous fishes including a shark, the latter being wrongly translated

lamprey by Willis. Harvey knew that some fishes congregate at stated seasons to breed. Of the Amphibia the frog and toad were Harvey's favourite species and he investigated the growth and hatching of the eggs. He said he has often dissected the tadpole and seen the gut, liver, and the pulsating heart. Also he corrected Fabricius' observations on the head and eyes.

In serpents Harvey noted and understood the posterior position of the heart and bladder-like lungs which so confused the medical anatomists in 1928 when preparations were being made to celebrate the discovery of the circulation of the blood. Harvey studied the development of the tortoise and found it to resemble that of the chick. In spite of such differences as occur in the development of the viper, which is viviparous, Harvey believed that the development of the chick, and the conclusions to be drawn from it, agreed with the generation of all other oviparous animals whatsover.

In the birds Harvey re-discovered the air sacs, probably known to Aristotle and described in modern times by Coiter in 1572. Early embryos of birds, mammals, and even man are so similar "that you cannot distinguish them." Copulation and hatching in the ostrich, bursa Fabricii, and anatomy of the cloaca in the Fowl. The early embryos of a number of species of mammals, birds, and reptiles, especially red-blooded forms, are so like "maggots" that they are indistinguishable from each other. Harvey quotes Aristotle that in Persia a pregnant mouse was found to contain female embryos that were also pregnant. Harvey comments that they were mothers before they were born. He, and other commentators, are, however, not quoting Aristotle correctly, for the latter only implied that the foetuses *appeared* to be pregnant.

As in the case of the oviparous species Harvey now affirmed that there is also a common type of development in the viviparous series, as in the horse, ass, ox, and hog. Structure of the early foetus in man and sheep. Parturition in many mammals and viper. Placenta in many mammals, mostly quotations from Fabricius.

It is often stated that Harvey was stimulated to the discovery of the circulation of the blood by Fabricius, his teacher at Padua, who had published an important suggestive work on the valves of the veins in 1603. Harvey left Padua and returned to England in 1602, but there is no reason to doubt his familiarity with the works of his instructor. Nevertheless, it was not until fourteen years had elapsed that he wrote the brief but unmistakable reference to the

circulation which appears on the verso of folio 80 of his 1616 lectures.

Harvey's 1616 lecture notes, brief and mystifying as such jottings must be to all but the lecturer himself, admit of no conclusion but that he was the pioneer of the experimental method in biology. To him the animal was a machine, and he wanted to know how it worked. Hence we see him studying the structure and movements of the heart from the simplest to the most complex of living organisms and finally reaching the conclusion to which his experiments directed him.

On Translating Harvey

KENNETH J. FRANKLIN*

IN 1932 at Dr. John F. Fulton's suggestion, I translated Richard Lower, *Tractatus de corde, item de motu et colore sanguinis et chyli in eum transitu,* 1669; in 1933, similarly Girolamo Fabrizi of Acquapendente, *De venarum ostiolis,* 1603; at his third suggestion, conveyed to me in 1955 as an invitation from Sir Walter Russell Brain, Bt., President of the Royal College of Physicians, I passed on to William Harvey, *Exercitatio anatomica de motu cordis et sanguinis in animalibus,* 1628, and made a new English version of that work.

So far so good, and unexceptionable. When, however, Dr. Fulton further requested this present paper or one on similar lines, my first reaction was that it would be unduly immodest to comply, and it was not until much later, when I had realized how much such statements from my predecessors would have helped me to assess the past story, that my natural reluctance in the matter was overcome in the interest of my readers and of any successors I may have as translator.

My first predecessor was, and remains, nameless, but his version first appeared in 1653, four years before Harvey's death; unfortunately, we do not know if it was acceptable to the writer of the Latin original. It covered not only the 1628 book, but also *Exercitationes duae anatomicae de circulatione sanguinis: ad Joannem Riolanum filium, Parisiensem,* 1649; it was printed by Francis Leach for Richard Lowndes of London and contained a preface by Zachariah Wood of Rotterdam. After twenty years it was reprinted; finally, in 1928 a limited new edition was issued through the Nonesuch Press by Geoffrey Keynes who stated that, for various reasons, he preferred this first translation to the subsequent, better known one by Robert Willis.

The second translator, who confined his efforts to the 1628 work, was one of three or four, more or less contemporary Michael Ryans practising or connected with medicine, and I am indebted in the main to Dr. Noël Poynter, of the Wellcome Historical Medical Library, for the following account of the Ryan in question.

He was born in 1800, perhaps in or near Dublin, where he

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later went to school. About 1817 he transferred to Edinburgh, where in 1821 he graduated M.D. with a thesis, *De genere humano ejusque varietatibus*. He also became a Member of the Royal College of Surgeons in Edinburgh. Thereafter he appears to have settled in London and to have begun contributing papers to the medical periodicals, likewise to have become Member of the London College of Surgeons and Licentiate (see the List of 1839) of the College of Physicians. From 1832 onwards he was Editor of one of the two *London Medical and Surgical Journals* and it was in that one that he published, in 1832-3, his translation of *De motu cordis*, which—one must admit—was not at all outstanding. Ryan according to J. F. Clarke (*Autobiographical reflections of the medical profession*, London, 1874, p. 280) "was a well-informed man—indeed a kind of medical dictionary—and withal a gentleman," and a study of his writings shows him to have been a man of wide reading with considerable linguistic ability and power of marshalling evidence. He died after a long illness which may have been tuberculosis.

The third translator was Robert Willis, who became M.D. Edinburgh like Ryan, but in 1819 (when he was only about 20), M.R.C.S. London in 1823, and L.R.C.P. London in 1827. From 1828 to 1845 he was Librarian of the Royal College of Surgeons of England and thereafter practised in Barnes until 1878, the year in which he died. By 1845 he had not only rendered signal service to the College in his capacity as Librarian, but had produced four original publications, three translations from the German, and two from the French. In 1845, when the Sydenham Society wished for someone who would translate Harvey's Latin works, Willis was chosen. He completed his task fairly rapidly, and his version appeared first in 1847; it was reprinted in 1889 and 1894 and, as a volume in *Everyman's Library*, first in 1907. To date, it is the standard rendering.

For the tercentenary in 1928, a "modern English version" of *De motu cordis*, etc. was made and annotated by Chauncey D. Leake, and published by Charles C Thomas; it has since undergone a number of reprintings. It was based, we are told, on the 1697 Longhine edition of Harvey's book; as this has not the dedications, Dr. Leake used Willis's version of these. The new "translation," based upon that of Willis, was said to be "in the language and spirit of our times," and "admittedly free, in the deliberate attempt to present Harvey's thought in the current physiological

manner." It is also said to have been "an easy task," with Willis as a guide. Personally, I have derived much information from some of the annotations with which Dr. Leake embellished his work. I do not, however, regard his text proper as a translation, but rather as a paraphrase which glosses over many difficulties.

Willis' translation has been praised by many for the grandeur of its style, but it is not infrequently slipshod in respect of the accepted rules of scholarship in translating, and its grandeur is on occasion reminiscent of relatively meaningless, if euphonious, passages in the authorized version of the Bible. The anonymous 1653 translation is not always easy to comprehend, but seventeenth century English suffers often from that drawback, and the translator may not fully have understood the new views which he was putting into the vulgar tongue a mere generation after their first printing.

I should temper my criticisms of the earlier versions by saying that I have also been much helped by them, even if my rule has been to make my own first translation of each successive section before seeing what my predecessors have written. However, the very fact that I accepted Sir Russell Brain's invitation shows that I was not satisfied with what was already available. Apart from objections already mentioned, all three of the earlier versions, when rendering the terms *vena arteriosa* and *arteria venosa,* turn them into *pulmonary artery and pulmonary vein*[*s*] respectively. Hence, to avoid a small extra mental effort on the part of the reader, they sacrifice the telling climax to which Harvey built up in the penultimate paragraph of his last Chapter. While on the subject of terminology, I should also stress that for Harvey the heart implied the ventricle[s] only, even if he brought the auricle[s] into his total picture of the blood propulsion, while *venae* in a few places indicated blood vessels in general rather than veins in particular. *Porositates* have some analogy with our capillaries, though the first publication about the latter was that which Malpighi produced in 1661, some time after Harvey's death. The word *anastomoses* is another which is difficult to translate so as to cover what one imagines Harvey meant by it. "Tricuspid mitral valves" have to some extent to be glossed over, and the wrong lettering of figures in Chapter Thirteen to be rectified.

My own main qualifications for attempting a translation of *De motu cordis,* etc. are an early and, I could say, strenuous classical education, many years of research upon the structure and func-

tioning of the cardiovascular system, and the experience of translating stated in the first paragraph of this paper. The early classical education gave me certain standards of scholarship which have on the whole persisted despite subsequent reduction, over a period of forty years, in my memory of the classical texts themselves. Unfortunately, those texts did not include the works of Aristotle, whose researches and philosophy made such an impression upon Harvey. His ideal translator also needs much else in his background, and in the natural course of things my version should, like its predecessors, eventually be superseded by a better; this happening may, however, be delayed because the number of those versed in both medicine and classics becomes constantly smaller with the passage of the years.

Since translations have for so long been the only way in which most people could get detailed knowledge of the new story which in 1628 Harvey had to tell, it is obvious that, provided my dissatisfaction with earlier versions was justified, I should have obtained a better insight into a most important character, and into a major portion of his work, than almost all before me. For, as I translated, I must have seen appearing in English the nearest approach yet in that language to what Harvey had intended to convey. I did, in fact, feel very conscious of this, and it added a thrill to translating and acted as a stimulus to me to make that translating as good as I possibly could. In addition, however, to becoming better informed about his message, I also became more aware of his dynamic approach and combatant spirit than I had ever become during the reading of earlier versions, and I kept being reminded of the Harvey pictured by Aubrey—"short of stature, dark of skin, with shining black eyes and hair, alert and irritable, often fingering the handle of his dagger."

Here, to illustrate the point, are the four translations of a certain sentence in the Introduction. Harvey's contemporary wrote, "But by my troth there are no such pores, nor can they be demonstrated." Ryan followed with, "But, in fact, there are not such porosities, nor can they be demonstrated," while Willis gave the rendering "But, in faith, no such pores can be demonstrated, neither in fact do any such exist." My own version was, "But, damme, there are no pores and it is not possible to show such." It took me two hours to think of "damme" as a sufficiently dramatic translation of "mehercule," but the *Oxford English Dictionary* then assured me that in the form "Damn me" it was in use as early

as 1645, and I felt that the two hours had been well spent in getting the effect that Harvey had, to my mind, intended by his use of a particular Latin word. I could readily multiply instances.

The picture drawn by Aubrey also seems to fit one who could produce the staccato passage introduced near the beginning of Chapter Five with consequent increase in *tempo*. It reads, "This is comparable with what happens in machines in which, with one wheel moving another, all seem to be moving at once. It also recalls that mechanical device fitted to firearms in which, on pressure to a trigger, a flint falls and strikes and advances the steel, a spark is evolved and falls upon the powder, the powder is fired and the flame leaps inside and spreads, and the ball flies out and enters the target." The passage as a whole has the rapid sequence that Demosthenes from time to time secured in his speeches by the deliberate introduction of a couple or so of hexameters, and I see it as one of a number of examples of Harvey's dynamic personality coming through into the very words with which he continued his unremitting efforts to convince his contemporaries of the truth which he had found. I shall be very happy if my new version does for others what it has done for myself, namely, makes them more conscious of companionship with one of our greatest scientists. During recorded history there has been no obvious increase in human intellectual powers, and it is indeed wonderful that we can come nearer in thought to one so great as Harvey merely by a more correct translation of his Latin.

I could presumably, through sufficient reading of contemporary work, have produced a translation in seventeenth-century-style English, but I could not see that any useful purpose would be served by such a procedure, and I therefore contented myself by using such devices as evoked favourable reviews of my earlier translation of Richard Lower's 1669 book. There is no need to detail these; I should, however, add that I frequently made use of English words prevalent in Harvey's time rather than of ones of definitely later date.

The 1628 book has been translated first as that was all that could be attempted in time for publication in June 1957, that is, for the tercentenary of Harvey's death. I trust, however, that the other contributions made by him to the same circulatory field will also be translated afresh, and that a scholarly commentary on all his functional cardiovascular studies will follow. I am not proposing myself for either task or excluding myself from them, but I look

forward very much to the completion of both and the better understanding of Harvey and of his work that should ensue.

By gracious permission, I have dedicated my translation of *De motu cordis,* etc. to the President and Fellows of the Royal College of Physicians, to the Treasurer and Governors of the Royal Hospital of St. Bartholomew in the City of London, and to the President and Governors of the Medical College of St. Bartholomew in the City of London.

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The Harveian Tradition in Scotland

DOUGLAS GUTHRIE*

ALTHOUGH William Harvey was by birth an Englishman and by choice a London physician, his travels and his discovery made him a citizen of many lands, and his name came to be held in grateful memory by every disciple of Aesculapius throughout the civilised world. The reception of his discovery in various countries has been the subject of intensive study, and it may be opportune in this tercentenary year of his death to reflect upon Harvey's associations with Scotland, a country so renowned in the annals of medical education. Not only did Harvey visit Scotland on two occasions, but his discovery was championed in 1701 by Archibald Pitcairne, the leading Scottish physician of his time, and since the end of the eighteenth century Harvey has been commemorated by the annual Harveian Festival (and Oration) at Edinburgh. Let us glance briefly at some aspects of Harvey's influence upon the progress of Scottish medicine.

Harvey's Visits to Scotland

Harvey, as Physician to the King, made his first journey to Scotland[1] in company with Charles I who had ascended the throne of England in 1625 and came north in 1633 to be crowned King of Scotland at Holyrood Palace in Edinburgh. Also of the party was Archbishop Laud whose *Liturgy* gave such offence to the Scots. At that time, the King regarded both State and Church as subject to his authority, and this attitude initiated those bitter quarrels with both his English and Scottish subjects, which precipitated the Civil War and eventually led to the King's execution in 1649. At the time of the Scottish Coronation the Court remained in Edinburgh for several weeks, and Harvey made good use of his spare time by visiting the Bass Rock to study the nesting habits of the Gannet or Solan Goose, which breeds on the Rock in great numbers. He describes the visit at some length in his *De generatione*, 1651.

* Edinburgh, Scotland.

1 Harvey's Scottish journeys are fully discussed by Sir Henry Wade in his Harveian Oration for 1938: Harvey in Scotland. *Edin. med. J.*, 1938, *45*, pt. 1, 761-781.

Various honors were bestowed on the visitors in Edinburgh. Along with fifteen other members of the King's entourage, William Harvey was admitted to the Freedom of the City; and the Royal College of Surgeons, or Incorporation of Surgeons, as it was then called, conferred Honorary Membership in the College upon three of the Surgeons to the Royal Household. The Royal College of Physicians could not honour Harvey in like manner, since this College was not founded until 1681.

King Charles I again visited Scotland in 1641, this time in a much more chastened and conciliatory mood. William Harvey, as his physician, accompanied him as far north as Aberdeen. Of this visit there are only scanty records, but we do know that Aberdeen followed the lead of Edinburgh in giving to Harvey the freedom of the city.[2]

Little more can be said of Harvey's travels in Scotland. It would have been interesting to know what his Scottish colleagues thought of his discovery. Nevertheless, it is probable that few of them, if any, were sufficiently qualified to judge of its merits, for Scotland did not become a centre of medical learning until the following century.

Scottish Critics and Supporters

The full purport of Harvey's discovery of the circulation of the blood was not apparent until many years after his publication of *De motu cordis* (1628).

One of his early (1630) critics, who received a good deal of publicity although he merely argued without repeating any of Harvey's experiments, was James Primrose of Hull. Of Scottish descent, he was a grandson of Gilbert Primrose, who was Deacon of the Incorporation of Surgeons at Edinburgh in 1581-82 and again in 1602, and Surgeon to King James VI and I. James Primrose is mentioned here merely as a remote Scottish link with Harvey; as a physician he, like so many others of his time, persisted in basing his statements upon those of Galen and the ancients.

There also lived in Harvey's day an anatomist of Scottish origin, Alexander Read of Aberdeen, who had gone to London to become lecturer on anatomy at the Barber Surgeon's Hall. In his *Manuall of the anatomy*, 1634, Read adhered to the old ideas and

2 Souter, W. C. Dr. William Harvey and Aberdeen. *Aberdeen University Review*, November, 1931.

did not even mention Harvey's work. His name is mentioned to illustrate the fact that anatomists were so curiously slow to change their views. One of the first of them to do so, by his acceptance of Harvey's discovery, was Nathaniel Highmore, who dedicated his textbook of anatomy to Harvey in 1651. The general acceptance of the new view of the circulation was long delayed, and it is not surprising that in Scotland, a very poor country in the seventeenth century, it was the subject of little printed comment.

Very few Scottish medical works appeared during the seventeenth century. Mr. Leonard Jolley[3] has given a list of twenty-four medical books by Edinburg, Glasgow, and Aberdeen authors, published before 1681 (the date of foundation of the Royal College of Physicians). These works deal mainly with medicinal springs and herbal remedies. One of the most famous physicians of the century, Sir Robert Sibbald, who was largely instrumental in founding the College of Physicians, had literary interests, but these were historical rather than medical as is shown by his chief work, *Scotia illustrata* (1684).

It was left to Archibald Pitcairne, Sibbald's junior colleague, to champion the cause of Harvey, and, in his *Dissertationes medicae* (1701), to show by mathematical reasoning that all the blood which reached the organs and the periphery returned to the heart. By this time the existence of a movement of circulation was accepted, but it was argued that only part of the blood returned by way of the veins. Pitcairne deserves credit for being the earliest Scottish writer to accept Harvey's discovery and for proving the importance of the value of mathematics and statistics in the solution of medical problems, a method upheld by Louis of Paris a century later, and, quite recently, applied still more closely to modern medicine.

Pitcairne did not live to see the establishment of the Edinburgh Medical School which he had envisaged. When he taught for a year at Leyden, in 1692, it is said that Boerhaave attended his lectures. The founders of the Faculty of Medicine of Edinburgh University, Alexander Monro primus and his four colleagues, were all pupils of Boerhaave, when he, in turn, was teaching medicine at Leyden. Thus were the ideas, so dear to the mind of Harvey, transmitted to the Scottish school through Pitcairne, Monro, and others.

[3] Jolley, L. 'Appendix' B, to *Report of the Scottish Society of the History of Medicine*, 1953-54.

The Harveian Society of Edinburgh

Scotland still honours the memory of William Harvey at Annual Harveian Festivals. Since 1798, no fewer than 124 Orations have been given, and a brief description of them may fitly conclude this attempt to assess the impact of Harvey upon Scottish medical education.

Dr. Andrew Duncan (1744-1828), a man of great personal charm and warm conviviality, was twice President of the Royal College of Physicians of Edinburgh. Succeeding James Gregory in the Chair of Medicine at Edinburgh University, he founded a number of societies, including the Aesculapian Club, which held its first meeting in 1773, the year which was believed to mark the 5000th Anniversary of the birth of Aesculapius. The number of members was originally limited to fifteen and later rose to twenty-two, consisting of Fellows of the Colleges of Physicians and Surgeons. This Club still flourishes as a dining club, and membership is regarded as a high honour. Early in its existence, the Club offered a prize for the best essay on a medical subject written by a student. This award came to be known as the Harveian Prize and consisted of a gold medal which, however, was presented only to the first winner. Later, the Prize was a set of Harvey's works and a sum of money. The first to win this competition (1777) and, on this occasion, to be awarded the gold medal was Charles Darwin, son of Erasmus Darwin of Lichfield, for his essay on the means of distinguishing between mucus and pus. Had he lived until 1809, he would have been the uncle of the more famous Charles Darwin. Unfortunately, he died at the age of nineteen from a dissection wound, and his grave may still be seen in the burial place of his teacher Andrew Duncan, who awarded to him and to four other students at varying times this posthumous hospitality.

In 1782 the farseeing Andrew Duncan, realising the need for a Society with a wider membership, founded the Harveian Society of Edinburgh, originally called the Circulation Club. The number of members is unlimited, admission is by ballot only, and the object of the Society is "to commemorate the discovery of the Circulation of the Blood and to cherish a kindly feeling among members of the Medical Profession." This Society appears to have assumed, at an early stage, responsibility for the Harveian Prize Essay, offered at first to students, and later to practitioners as well.

Awards were made at irregular intervals until the year 1865, when they were discontinued.

ORATIONS AND COMMEMORATIONS

The main activity of the Harveian Society of Edinburgh, however, has always centred around its Annual Festival, which, formerly held on Harvey's birthday in April, is now held in May or June and consists of an Oration by the President, followed by a formal dinner. It is always a popular event, attended not only by Edinburgh members but by members residing elsewhere. In early times a President might continue to hold office for several years, but, as a rule, he is now appointed annually, and is chosen alternately from Edinburgh and some other place. The first Oration, entitled "De laudibus Guilielmi Harvei Oratio," was delivered by Andrew Duncan, who was President for eight subsequent years. This was the only Oration to be given in Latin, a striking difference from the Harveian Orations of the Royal College of Physicians of London, instituted by Harvey himself and always given in Latin until the middle of the nineteenth century.[4] There were a number of years in which no Oration was delivered in Edinburgh, including a break of sixteen years early in the nineteenth century, and another of eight years, during and after World War II, as well as some shorter intermissions. No rule has ever been applied regarding the title of the discourse, and it was usual, until the eighties of the last century, to commemorate the work of some famous man of the past, not always a medical man. Among the men thus honoured during the first century of the Society's existence, were Pitcairne, Monro I, Pringle, Gregory, Sir Henry Raeburn, and Sir Joseph Banks.[5] The life and work of Harvey was chosen by only five orators of this period out of a total of ninety-three Orations. Between 1890 and 1956 a philosophical subject of discourse was often chosen, and of the fifty-two Orations of this later period, twenty dealt directly with Harvey. The choice of a topic other than Harvey was perfectly justified, of course, and many an erudite

[4] The Orations in London are not to be confused with the lectures endowed by the Harveian Society of London in 1875. The Harveian Society of Edinburgh is almost a century older than the London Society. Neither Society has any official connection with the relative Royal College. There is also a Harvey Society of New York, which has sponsored lectures since 1905.

[5] Wemyss, H. L. Watson, *A record of the Edinburgh Harveian Society 1782-1933.* Edinburgh, 1933.

Oration has been given. Most of them have appeared in print. During the past twenty years there has been a return to Harvey, perhaps because of a feeling that the importance of his discovery and of his method of experiment combined with reason are not fully recognized by the present generation of medical men. Furthermore, each succeeding generation has its own approach to history, and the story of Harvey, familiar though it be, requires frequent restatement in the light of modern knowledge.

Scotland, which has played so noteworthy a part in medical education, may well revere afresh the memory of William Harvey in 1957, three hundred years after his death.

William Harvey: His Neurological and Psychiatric Observations

RICHARD A. HUNTER* AND IDA MACALPINE*

THE attempt to uncover new facets of Harvey's many-sided genius may seem like that of an earlier student who in 1787 searched Padua for "additional information" about Harvey's stay there, believing that "The road which led to the Circulation of the Blood, was not explored in the hypochondriacal geography, from Cambridge to Warwick-lane" but "in the happy climate of Italy. . . . After such a lapse of time, it was not likely that I should obtain any anecdotes concerning him. But I did not omit to inquire" (Moseley, 1807). In the same spirit we searched afresh for material reflecting Harvey's interest in and observations on the nervous system and the mind and body-mind relation—since separated into neurology, psychiatry, and psychosomatic medicine.

The many commentaries, orations, histories, and biographies have largely ignored if not denied that Harvey contributed to these fields. Sieveking (1877) searched "in vain for a sign that he even appreciated the importance of the nervous system"; while Savage (1909) concluded that "Harvey . . . does not seem to have been attracted . . . by mental diseases." The reason for such statements is perhaps that Harvey's notes lie scattered among his writings; he never treated them systematically nor put forward a theory.[1]

Yet it is hardly likely that so keen an observer of "the foundation . . . the sovereign" (Harvey, 1628) of human life, the heart, should not have been equally interested in human nature, its sensibilities and expressions. It is in fact most instructive to trace how motion, the centre of Harvey's interest, led him to turn his attention to its counterpart, sensation, and so from heart to nerves and nervous system: "the heart, with the veins and arteries and the blood they contain, is to be regarded as . . . the fountain and original of all things in the body, the primary cause of life; and this in the same acceptation as the brain with its nerves, organs of sense and spinal marrow inclusive, is spoken of as the one and general

* London, England.

[1] According to Sir Charles Scarburgh (Payne, 1957), among Harvey's papers which unfortunately perished during the Civil War were the results of his researches "on the brain . . . on the movement and feeling of animals."

organ of sensation" (Harvey, 1651). How these interests were fused in Harvey's mind is shown by his examination of Hugh Montgomery, later Earl of Mount Alexander, in whom an injury "attended with fracture of the ribs of the left side" had left "a very large open cavity" exposing the heart. Harvey (1651), not content merely to observe and feel his beating heart, proceeded to test sensation: "I cannot be silent on the remarkable fact, that the heart itself, this most distinguished member in the body, appears to be insensible. . . . I carried the young man himself to the king. . . . And his most excellent majesty, as well as myself, acknowledged that the heart was without the sense of touch."[1a]

Like Harvey's (1616) "Anatomist too curious; making mor parts then is," we have somewhat artificially divided our material into neurological and psychiatric topics, and prefaced each with a clinical anecdote preserved by his friends Sir Kenelm Digby and the Honourable Robert Boyle.[2] These epitomise and exemplify his abiding concern with these subjects.

Neurology

Modern neurology developed from two fundamental discoveries—to each Harvey contributed: firstly, the distinction between motor and sensory nerves and, secondly, the differentiation of irritability from sensibility and contractility.

Motion and Sensation

Digby (1644) in his *Two Treatises* discussed whether the same nerves could conduct both motion and sensation and joined issue with Descartes (1637; 1641) who held that spirits sent from brain to muscle along nerves caused motion, and that the same nerves passively transmitting a vibration to the brain produced sensation. In Chapter 32, "Of sensation," in the section entitled, "That the symptomes of the palsie do no way confirme Monsieur des Cartes his opinion," Digby wrote:[3]

The Case of "a seruant in the colledge of Physitians in London"

Monsieur des Cartes endeauoureth to confirme his opinion, by what vseth to fall out in palsies, when a man looseth the strength of mouing his

[1a] Many years later Boyle (1684) followed up this "*British* Nobleman, of whom our famous *Harvey* tells a memorable, not to say matchless, story": he "grew a strong, and somewhat corpulent man; and . . . marryed one of my nearest kinswomen."

[2] That these cases aroused considerable interest is shown by their inclusion in Wanley's (1678) encyclopaedic *Wonders of the little world* which originally directed our attention to them.

[3] This passage was first noticed by Fulton (1937).

handes, or other members, and neuerthelesse retaineth his feeling: which he imputeth to the remaining intire of the stringes of the nerues, whiles the spirits are someway defectiue. To this we may answere, by producing examples of the contrary in some men, who haue had the motion of their limbes intire and no ways preiudiced, but haue had no feeling at all, quite ouer their whole case of skinne and flesh: as particularly a seruant in the colledge of Physitians in London, whom the learned Haruey (one of his Masters) hath told me, was exceeding strong to labour, and very able to carry any necessary burthen, and to remoue thinges dexterously, according to the occasion: and yet he was so voyde of feeling that he vsed to grind his handes against the walles, and against course lumber, when he was employed to rummage any; in so much, that they would runne with bloud, through grating of the skinne, without his feeling of what occasioned it.

That Harvey told Digby of this patient shows not only that he had recognized an uncommon condition—probably syringomyelia (Hunter and Macalpine, 1956*a*)—before similar cases were described by other physicians, but also, and more important as appears from the context in which Digby quoted it, he had recognized the significance of the patient's symptoms for understanding the functioning of the nervous system. In this he was in advance even of Thomas Willis, "the first inventor of the *nervous* system" (Freind, 1727), who decades later saw no reason to doubt the accepted notion of two-way traffic in nerves: "If it be demanded, why sense is not always hindred as well as motion in every *Palsie*, since as it seems either is performed by the same Nerves and Fibres, within the same Medullary tracts, so that one faculty is only the inversion of the other? as to this we may say, that as light beams thorow glass, when wind is excluded, so also sense being safe, oftentimes motion is lost" (Willis, 1672).

The next to follow Harvey's trend of thought was a friend of both Harvey and Digby, Walter Charleton. Basing himself firmly on the work of "*our immortal* Harvey" he wrote: "we do not find, that Nature hath any where in the Body instituted contrary Motions of the same *Fluid* in the same Vessels; unless in the branches of the Wind-pipe, and in the Guts. . . . in the *Nerves* there occurs no sufficient reason, why the Spirits should be carried sometimes outward, and sometimes inward; unless we attribute to them a Peristaltick motion, or imagine that the Spirits themselves, like Gnats playing in the Air, fly to and fro in the Nerves, at their own pleasure" (Charleton, 1680).

Although Harvey did not establish that the simple principle of one-way traffic—the essence of his discovery of the circulation of the blood (Cole, 1955)—was also the key to the nervous system, his

clinical observations nevertheless led him to doubt the then accepted tenet of sense and motion being conducted by the same nerves. He thus broached a fundamental problem of neurology, and so pointed the road to progress. Only when almost two hundred years later the functional and anatomical distinction between motor and sensory nerves was established (Bell, 1811; Magendie, 1822) could scientific neurology begin. Hence Bell's discovery of one-way traffic in nerves has been rated second only to that of Harvey (Neuburger, 1897).

Conduction of Nervous Impulses

Harvey also broke away from contemporary concepts such as animal spirits and nervous fluids flowing through the nerves. In a passage of the finest Harveian reasoning he scathingly referred to "Persons of limited information, when they are at a loss to assign a cause for anything, very commonly reply that it is done by the spirits" (Harvey, 1649). He then passed in review the three kinds of spirits usually invoked: "the natural spirits flowing through the veins, the vital spirits through the arteries, and the animal spirits through the nerves." But "besides the three orders of influxive spirits . . . a like number of implanted or stationary spirits seem to be acknowledged; but we have found none of all these spirits by dissection. . . . They who advocate incorporeal spirits have no ground of experience to stand upon; their spirits indeed are synonymous with powers or faculties, such as a concoctive spirit, a chylopoietic spirit, a procreative spirit, &c.—they admit as many spirits, in short, as there are faculties or organs."

Irritability—Involuntary Motion—Reflex Action

Charleton (1680) also credited Harvey with having distinguished what he "call'd by the name of *Tactus Naturalis*" or "*Natural sense* of *Touching* whereof we are not conscious . . . from the *Animal Sense* of *Touching*, whereof we alwayes are conscious." This was later developed into the concept of irritability by Glisson who "coming after to consider the thing more Metaphysically, and founding the very life or substantial Energie of Nature wholly upon the same, denominated it *Perceptio Naturalis*, thereby to distinguish it from all the Senses, as well internal, as external" (Charleton, *loc. cit.*). Harvey (1651) had written:

> All sensation and motion do not proceed from the brain. . . . The motions and actions which physicians style *natural*, because they take place involuntarily, and we can neither prevent nor moderate, accelerate nor

retard them by our will, and they therefore do not depend on the brain,[4] still do not occur entirely without causing sensation, but proclaim themselves subject to sense, in as much as they are excited, irritated, and altered[5] thereby . . . it is to be concluded that there is a certain sense or form of touch which is not referred to the common sensorium, nor in any way communicated to the brain, so that we do not perceive by this sense that we feel . . . which we therefore distinguish from the proper animal sense.

The "divine Banquet of the brayne" (1616)

Harvey's (1616) *Prelectiones anatomiae universalis* remain a large and mostly untapped source of information about his ideas, experiences, and observations in all branches of medicine, pathology, anatomy, and physiology—both human and comparative. There are frequent references to the structure and function of the nervous system: of 98 folios the last 13 are entirely devoted to it, and "Cor et Cerebrum" are frequently considered together. Among the neurological conditions mentioned are hydrocephalus, epilepsy, apoplexy, paralysis, loss of sensation, blindness due to cerebral disease, delirium, phrenitis, and coma. Like many of his contemporaries Harvey marvelled at the fact that the brain

"Bipartitum dextrum et sinistrum sed hic Mirum
leso enim dextra parte paralysis sinistrae et contra."

Throughout every organ is considered in relation to the nervous system. Thus in his discussion of the spleen:

"Nervulos aliquando sinistro latere ab ijs qui lienem
vnde Consensus Cerebro Magnus et contra
vnde vulneratis vomitus et Hemicranea
vnde affecto patitur Cerebro Imbecillitatem Melancholiam."

He was familiar with the "Cerebri pulsus" and that coughing, convulsions, and screaming

"pertumescit Cerebrum. . . .
WH vnde cautio in trepanning one side
being thorow before the other.
vnde not when the brayne swells. coughs."

[4] This passage shows that Harvey used the concept of "involuntary" as distinct from "voluntary" motion before Willis (1672) to whom credit is usually assigned.

[5] We have throughout followed Willis's (1847) translation of Harvey's works except in this instance. The original Latin reads *"excitentur, irritentur et permutentur,"* which Willis translated as "aroused, called forth, and changed." However, as the term "irritability" is usually ascribed to Glisson (1654; 1677), for example by Foster (1901), it seemed important to point out Harvey's use of the word.

In this connection it is of interest to note his suggested treatment of Lady Anne Conway (Hunter and Macalpine, 1956*b*) who had suffered for many years from persistent and severe attacks of headache:

> There is yet another Chirurgical operation [wrote her later physician Willis (1672)] cry'd up by many for a pertinacious Headach, but by none (that I know of) yet attempted, to wit, an opening of the Skull, near the grieved place, with a *Trypaning* Iron. This our most ingenious *Harvey* endeavoured to persuade a Noble Lady, labouring with a most grevious and inveterate Headach, promising a Cure from thence; but neither she, nor any other would admit that administration.

Psychiatry

Robert Boyle's (1688) account of "the only Discourse I had with . . . our famous *Harvey*" during which he asked Harvey "What were the things that induc'd him to think of a *Circulation of the Blood?*" is so well known (Lawrence, 1766; Fulton, 1932) that it has led to an earlier and different version of their meeting being overlooked. Boyle (1663) recounted that when he had gone to consult

> our most experienced *Harvy* . . . about my weak Eyes, he told me, among other things (as a very remarkable one) that he had once a Patient (whose Name and Profession he told me, but I remember not) that had a confirm'd Cataract in his Eye, and yet upon the use of Physick, to which he could not ascribe so wonderful an effect, that Cataract was perfectly dissipated, and the Eye restored to its wonted Function.[6]

[*The Case of the Maid who had "lost the sense of feeling"*]

> Which brings into my minde another Observation, imparted to me, a while since, by that excellent and experienc'd Lithotomist, Mr. *Hollyer,* who told me, that among the many Patients sent to be cured in a great Hospital[7] (of which he is one of the Chirurgions) there was a Maid of about eighteen Years of age, who, without the loss of motion, had so lost the sense of feeling in the external parts of her Body, that when he had, for tryal sake, pinn'd her Handkerchief to her bare Neck, she went up and down with it so pinn'd, without having any sense of what he had done to her. He added, That this Maid having remained a great while in the Hospital without being cured, Dr. *Harvey,* out of Curiosity, visited her sometimes; and suspecting her strange Distemper to be chiefly Uterine, and curable onely by *Hymeneal* Exercises, he advised her Parents (who sent her not thither out

[6] Cf. "one of the eminentest Doctors of the *London Colledge* assur'd me, that he had, with one of these stones . . . said to be found in the Heads of a certain kind of Serpents about *Goa* . . . done, tho contrary to his expectation, a notable Cure, which he related to me at large" (Boyle, 1685).

[7] Thomas Hollier was surgeon to St. Thomas's Hospital, London, and later lithotomist to St. Bartholomew's, and is mentioned in the Diary of his friend and patient Samuel Pepys.

of poverty) to take her home, and provide a Husband, by whom, in effect, she was according to his Prognostick, and to many Mens wonder, cur'd of that strange Disease.

This patient was also noticed in *Historia LXXXII* under *Sensus Abolitus* by Thomas Bartholin (1657) who possibly heard about her from Harvey himself: "Aliam virginem sanam in Anglia novit *D. London* Medicus,[8] amicus olim meus, qvae ustiones in collo non sentiebat, acumq́; & fronti impressam & unguium radicibus intrusam sine ullo doloris sensu admisit."

Furor Uterinus

That Harvey at once recognized that Hollier's patient was not suffering from some dread bodily disease although she had "remained a great while in the Hospital without being cured" shows that he was familiar with the condition. Indeed in 1616 he had noted similar cases:

". . . Nan Gunter[9] &c. puto callum
fecisse the mad woman pins in her arme
Mary pin her cross-cloth . . ."

In 1651 he again mentioned some patients "who are deranged in mind, or who are agitated to such a degree by violent passion that they feel no pain, and pay no regard to the impressions made on their senses." The cause of her becoming "deranged in mind" was that some "young women . . . when they become marriageable . . . if they continue too long unwedded, are seized with serious symptoms—hysterics, furor uterinus, &c. or fall into a cachectic state, and distemperatures of various kinds" due to "their eagerness for offspring. . . . In like manner women occasionally become insane through ungratified desire, and to such a height does the malady reach in some, that they are believed to be poisoned, or moon-struck, or possessed by a devil" (Harvey, 1651). Hence his advice to "provide her a Husband, by whom, in effect, she was . . . cur'd."

[8] We assume that "*D. London* Medicus" refers to Harvey, "a physician of London," although literally it means "Master London, physician." However, there was a contemporary physician, Richard London, *Anglus Norfolcensis*, who graduted M.D. Padua 2 October 1640 (Morpurgo, 1927), whom Bartholin may have met on his travels through Europe. That Bartholin thought highly of Harvey and his work may be seen from the one letter which remains of their correspondence (Bartholin, 1663). Harvey's only reference to Bartholin (letter to J. D. Horst, 13 July 1655) confirms the impression that they had been in correspondence.

[9] Transcribed in 1886 as "Nan gunter" but obviously a proper name, and possibly a relative of Edmund Gunter (1581-1626), Professor of Astronomy at Gresham College.

Hysteria

Harvey (1651) recorded two other patients with "prolapsus uteri": one "for more than ten years laboured under furor uterinus and melancholy," the other "suffered long with hysterical symptoms." In the pathology of the age he concluded "how dreadful, then, are the mental aberrations, the delirium, the melancholy, the paroxysms of frenzy, as if the affected person were under the dominion of spells, and all arising from unnatural states of the uterus." He also noted another manifestation: "WH Histericam passionem cum multo Risu Novi" (Harvey, 1616). These observations on so-called hysterical anaesthesia and paroxysms show that Harvey clearly recognized the signs and symptoms of the condition familiarized as "la grande hystérie" by Charcot at the Salpêtrière more than two hundred years later.

Folie à Deux and Pseudocyesis

Harvey (1651) in passing also gave one of the earliest clinical descriptions of shared delusions or *folie à deux* (Greenberg *et al.*, 1956): a woman with ten children married a second husband and became convinced she was pregnant again. She managed to convince her sister "who occupied the same bed with her," and "No arguments of mine could divest her of this belief. The symptoms depended on flatulence and fat." He also described another variety of pseudocyesis occurring in a childless patient "who experienced in her own person all the usual symptoms of pregnancy . . . calculated the time at which she expected her delivery . . . prepared the bed, cradle, and all other matters ready for the event. But all was in vain. Lucina. . . . tutelar deity of childbirth. . . . refused to answer her prayers; the motions of the foetus ceased; and by degrees, without inconvenience, as the abdomen had increased so it diminished; she remained, however, barren ever after."

Dreams and Insanity

The only discussion of Harvey's (1627) lectures "De Musculis, motu locali. &c." is that of Paget (1850), which—in the absence of a transcript—remains the sole published source of its contents. Under the heading "De Passionibus Musculorum sive motus depravat" are included "Histeric" convulsions, the tremor of "Horror," and "Inquietudo . . . obambulatio somno. decubitus vitiosus, mania, Vitae saltus," which led Paget to remark that "The grouping together of mania and somnambulism is interesting. It seems to

indicate that Harvey had recognized the resemblance between the states of dreaming and insanity."[10]

Harvey's Notes on Galen's "De . . . Animi Perturbationibus"

Harvey's interest in "affections of the mind" is clearly documented by his annotations in the British Museum copy of Goulston's (1640) edition of *Claudii Galeni Pergameni Opuscula Varia* (Fig. 1). The only mention of this volume remains Moore's (1908) announcement of his discovery. However, he was concerned with identifying Harvey's handwriting and describing his notes and underlinings rather than with the actual content of the annotations.[11] Of the seven treatises which make up the book, three are concerned with the mind, mental illness, and the body-mind relation: *De cognoscendis et corrigendis cujusque animi perturbationibus, De diagnoscendis et corrigendis cujusque animi erratis,* and *Quod animi mores sequantur temperamentum corporis.* These are extensively underlined and have marginal notes on almost every page, with such characteristic symbols as *WH, x* (for *exemplum)* and *NB* (Fig. 2). Moreover, at the end of the book, on the verso of the penultimate blank leaf, Harvey wrote comments on passages in *De . . . animi perturbationibus* giving the relevant page numbers to which these refer. This manuscript has hitherto not been published nor has it been transcribed. It is reproduced here (Fig. 3) because his careful study of this treatise illustrates how much thought Harvey gave to mental illness.

PSYCHOSOMATIC MEDICINE

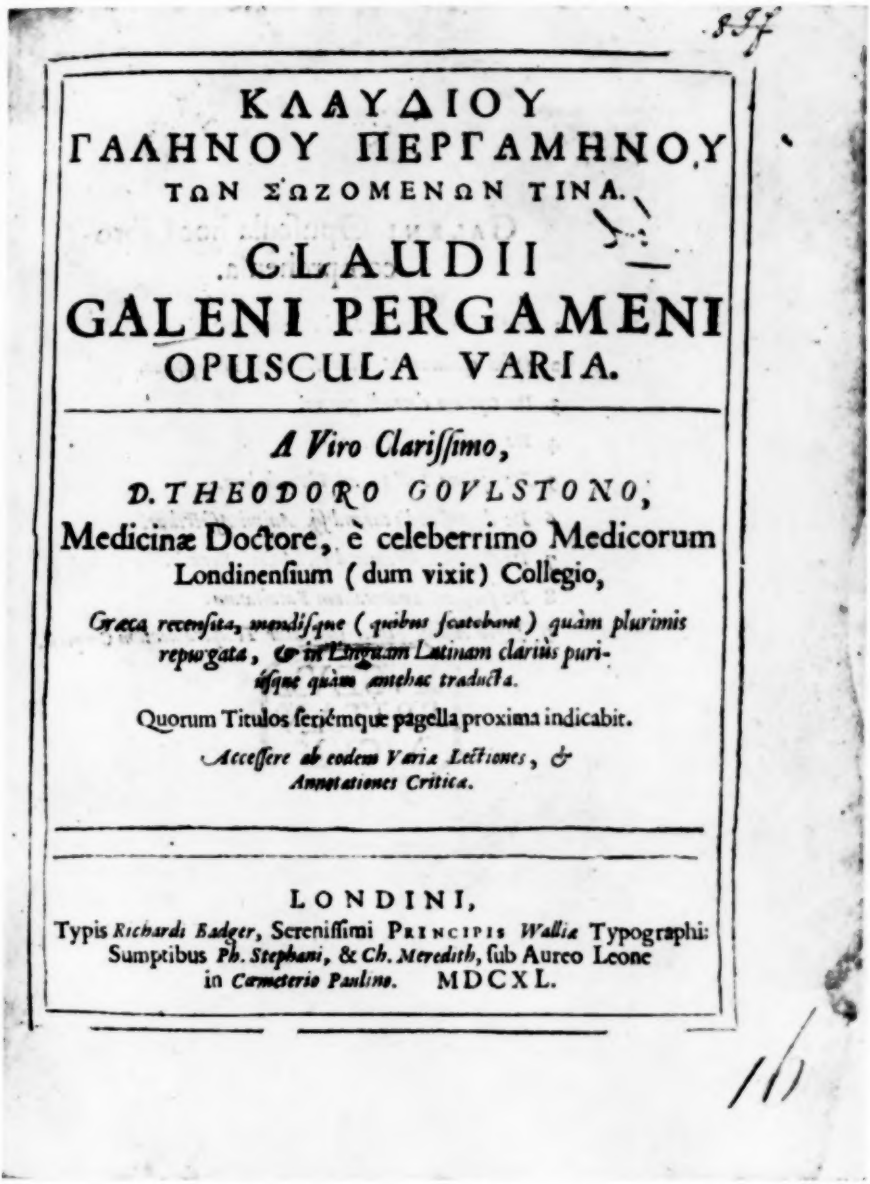
Even in *De motu cordis* Harvey (1628) considered the important influence of the mind upon the body and made an observation, the full import of which is only now being realized: "Every affection of the mind that is attended with either pain or pleasure, hope or fear, is the cause of an agitation whose influence extends to the heart. . . . And hence, by the way, it may perchance be wherefore

[10] At the end of the MS. Harvey raised queries about the functional relation of brain, nerves, and muscles in such terms as:

"An Cerebrum Master : Spina his Mate.
Nervi Boteswayne
Musculi Saylors."

There are "similar Comparisons of the Brain with a Military Commander, the Leader of an Orchestra, an Architect, and the *Prius Motor,* and of the Nerves and Muscles with the respective subordinate officers" (Paget, 1850).

[11] Moore's intention to publish "a full account" was never realized, and his son kindly informed us that "as far as can be discovered there is no MS. remaining, if ever written" (Moore, 1956).



ΚΛΑΥΔΙΟΥ
ΓΑΛΗΝΟΥ ΠΕΡΓΑΜΗΝΟΥ
ΤΩΝ ΣΩΖΟΜΕΝΩΝ ΤΙΝΑ.

CLAUDII
GALENI PERGAMENI
OPUSCULA VARIA.

A Viro Clariſſimo,
D. THEODORO GOULSTONO,
Medicinæ Doctore, è celeberrimo Medicorum Londinenſium (dum vixit) Collegio,

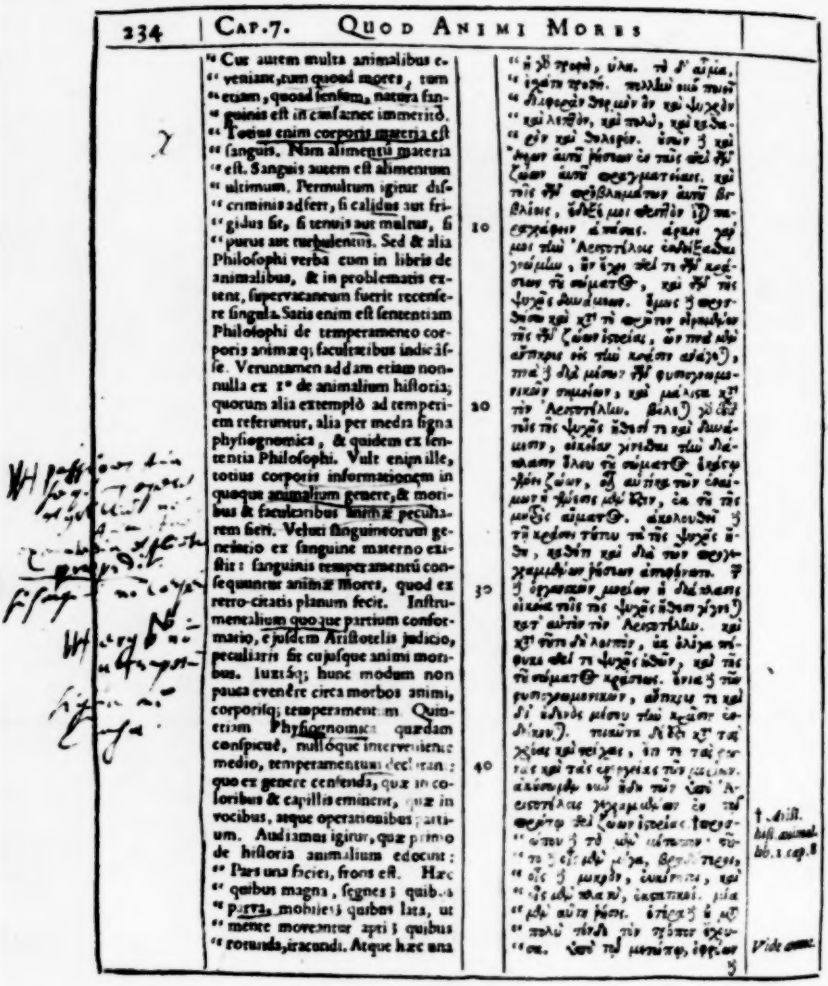
Græca recenſita, mendiſque (quibus ſcatebant) quàm plurimis repurgata, & in Linguam Latinam clariùs puriúſque quàm antehac traducta.

Quorum Titulos ſeriémque pagella proxima indicabit.

Acceſſere ab eodem Variæ Lectiones, & Annotationes Criticæ.

LONDINI,
Typis *Richardi Badger*, Sereniſſimi PRINCIPIS *Walliæ* Typographi:
Sumptibus *Ph. Stephani*, & *Ch. Meredith*, ſub Aureo Leone
in *Cœmeterio Paulino*. MDCXL.

FIG. 1. Title-page of Harvey's copy of Goulston's 1640 edition of Galen's *Opuscula Varia*, in the British Museum.



"Cur autem multa animalibus e-
"veniant, tum quoad mores, tum
"etiam, quoad sensum, natura san-
"guinis est in cujusnec immeritò.
"Totius enim corporis materia est
"sanguis. Nam alimentū materia
"est. Sanguis autem est alimentum
"ultimum. Permultum igitur dis-
"crimins adfert, si calidus aut fri-
"gidus sit, si tenuis aut multus, si
"purus aut turbulentus. Sed & alia
Philosophi verbā eum in libris de
animalibus, & in problematis ex-
tent, supervacaneum fuerit recensere singula. Satis enim est sententiam
Philosophi de temperamento corporis animæ qi facultatibus indicē esse. Verumtamen addam etiam nonnulla ex 1° de animalium historia,
quorum alia extemplò ad temperiem referuntur, alia per medra signa
physiognomica, & quidem ex sententia Philosophi. Vult enim ille,
totius corporis informationem in
quoque animalium genere, & moribus & facultatibus animæ peculiarem fieri. Veluti sanguineorum generatio ex sanguine materno existit: sanguinis temperamentū consequuntur animæ mores, quod ex retro-citatis planum fecit. Instrumentalium quo-que partium conformatio, ejusdem Aristotelis judicio, peculiaris sit cujusque animi moribus. Iuxtaq; hunc modum non pauca evenēre circa morbos animi, corporiq; temperamentum. Quinetiam Physiognomica quædam conspicuè, nulloque interveniente medio, temperamentum declarant: quo ex genere censenda, quæ in coloribus & capillis eminent, quæ in vocibus, atque operationibus partium. Audiamus igitur, quæ primo de historia animalium edocuit:
"Pars una fricti, frons est. Hæc
"quibus magna, segnes; quibus
"parva, mobiles; quibus lata, ut
"mente moveantur apti; quibus
"rotunda, iracundi. Atque hæc una

"ἡ δὲ πρωτὴ, ὑλη. τὸ δ' αἷμα,
"ἐσχατη πρωτη. πολλὴν οὖν πιοῦ
"διαφορὰν ἔσχεν ὁ τοῦ ἐν τοῖς
"καὶ λεπτὸν, καὶ πολὺ, καὶ καθα-
"ρὸν καὶ θολερὸν. ὅλου γὰρ καὶ
"ἁπλοῦ αὐτοῦ ὄντος ἐκ τοῦ αἵματος
ζώων αὐτὸ σκεύασματόιαι. καὶ
τοῖς μὴ σκληρόδερμα αὐτῶν βρ
βλίοις, ὅλως μοι φαίνεται ἡ πα-
σκευάσθαι αἵματος. ἀρκεῖ γάρ
μοι τῶν Ἀριστοτέλους εἰρημένων
γνώμῶν, ὃν ἔχει τέλ τι τὴν κεφά-
λαιον τῦ σώματος, καὶ μὴ τὰς
ψυχὰς δυνάμεων. ἅμως μ σκεπ-
τέον καὶ κ τὸ σκοπεῖν ἀφικνέῖσθαι
τὰς μὴ ζώων ἰδιότητας, ὧν πρὸ μὲν
αὐτῶν ἐν τῶν κεφαλαίων εἰρήκαμ),
πρὸ μὲν δὲ μόνων μὴ φυσιογνωμο-
νικῶν σημείων, καὶ μάλιστα κ
τὸν Ἀριστοτέλην. βέλτ) γὰρ
τὰς τὰς ψυχὰς ἤθεσί τι καὶ δυνά-
μεσι, οἰκεῖον γενέσθαι τῶν δια-
φορᾶν ἴδιον τῦ σώματος ἑκάστῳ
γένει ζώων, ἐξ αὐτῆς τῶν ἐναί-
μων ἡμῖν μὴ ὄντων, ἐκ τῶν τῆς
μυζῆς αἵματος. ἀκολουθεῖ δ
τῷ κράσει τύπῳ τὰ τῆς ψυχῆς ἤ-
θη, καθάπ καὶ διὰ τῶν σκλη-
ρομερῶν μορίων ἀποφήνατο. ἡ
ὀργανικῶν μερῶν ἁ διάθεσις
εἰκόνα τῆς τῆς ψυχῆς ἕξεως δ
τοῦ αὐτὸν τὸν Ἀριστοτέλην. καὶ
κ τῶν διὰ λεπτὸν, ὡς ἀλλοῖα πέ-
φυκε τέλ τι ψυχῆς ἤθεσι, καὶ τῆς
τῦ σώματος κράσεως. ὅτιαδ τῶν
φυσιογνωμονικῶν, ἀδηλῶ τι καὶ
δι' ἐλαχίστ μέσου τῶν κρώτων εὐ-
δήλων). τοιαῦτα δ' ἔστ κ τὰς
χρόας καὶ τρίχας, ἐν τε ταῖς φω-
ναῖς καὶ ταῖς ἐνεργείαις τῶν μορίων.
ἀκούσωμεν οὖν ἤδη τῶν ὑπὸ Ἀ-
ριστοτέλους γεγραμμένων ἐν τῷ
περὶ τῶν ζώων ἱστορίας τρίτῳ·
"ὧν δ τὸ μὲν πέρατον τῦ
"τὸ δ' ἐξ ἀνὰ μέσον, βραχ[illegible]τερον,
"ὃς δ μικρὸν, εὐπάτητον, καὶ
"ὃς μὲν πλατὺ, εὐκίνητον, μία
"μὲν αὐτῇ ῥώμη. ὁτῳοῦν δ μὴ
"πολὺ τοῦτ τῶν πρόσθεν ἐχεν-
"τα. ὑπὸ τῆ μετώπῳ, ὀφρύων

† Arist. hist. animal. lib. 1. cap. 8

Vide annot.

FIG. 2. An example of Harvey's annotations and underlinings in the treatise *Quod animi moves sequantur temperamentum corporis*, p. 234.

FIG. 3. Harvey's unpublished notes on the treatise *De cognoscendis et corrigendis cujusque animi perturbationibus* which occupies pp. 144-179 in Goulston's 1640 *Claudii Galeni Pergameni Opuscula varia*, written on the recto of the penultimate blank leaf of the British Museum copy.

grief, and love, and envy, and anxiety, and all affections of the mind of a similar kind are accompanied with emaciation and decay, or with cacochemy and crudity, which engender all manner of diseases and consume the body of man."

In 1649 he again referred to "the signal influence of the affections of the mind . . . what indeed is more deserving of attention than the fact that in almost every affection, appetite, hope, or fear, our body suffers, the countenance changes, and the blood appears to course hither and thither. In anger the eyes are fiery and the pupils contracted; in modesty the cheeks are suffused with blushes; in fear, and under a sense of infamy and of shame, the face is pale, but the ears burn as if for the evil they heard or were to hear; in lust how quickly is the member distended with blood and erected!" In illustration he retailed the story of a "strong man, who having received an injury and affront from one more powerful than himself, and upon whom he could not have his revenge, was so overcome with hatred and spite and passion, which he yet communicated to no one, that at last he fell into a strange distemper, suffering from extreme oppression and pain of the heart and breast. . . . Such is the force of the blood pent up, and such are the effects of its impulse."

In all sections of his anatomical lectures Harvey (1616) noted the relation of body and mind, especially the influence of the passions:

"passiones multae Inflammant phrenitidem."

Thus of the skin:

"praecipue passionibus { timore see the divil: &c. / Horrore steteruntque comae / frigus fla bello cowld / a goose scin."

Of the abdomen:

"contra Adaucto calore tenui Alimento
Melancholici phanatici flatulenti
vnde Melancholici Hypochondrici deficiunt Affectione ingenio."

Of the relation of the heart, liver, and brain:

"Item inter Cor et Jecur equilibrium
vt inter Jecur et cerebrum
vunde quanto sanitate et colore florido
tanto deficiunt operationibus Animi."

Of the kidneys:

"timore libidine piss often hors befor ye rase."

Of hysterics:

"Histericis cutt laces
gravidis item Repletis
they all vse this panting sighing
fashion of breathing."

Also mentioned are insanity, mania, hypochondria, alcoholism, imbecility, hallucinations, and such faculties of the mind as "phantasia" and "memoria"; "et Hoc necesse quo percipiatur idem esse quod videns audiens &c /pluribus aprehensibilibus vnum Intelligens."

Lost Writings

In several passages in his published works Harvey expressed his intention of writing more extensively and consecutively on "the influence of age, sex, temperament, and habit of body" on disease, as well as on "the signal influence of the affections of the mind. . . . But here I come upon a field where I might roam freely and give myself up to speculation. And, indeed, such a flood of light and truth breaks in upon me here; occasion offers of explaining so many problems, of resolving so many doubts, of discovering the causes of so many slighter and more serious diseases, and of suggesting remedies for their cure, that the subject seems almost to demand a separate treatise. And it will be my business in my 'Medical Observations' to lay before my reader matter upon all these topics which shall be worthy of the gravest consideration" (Harvey, 1649).

Elsewhere writing "On Conception" he observed that "The brain is the organ of one kind of conception" comparable to "the 'conception' . . . of the uterus or the ovum. . . . But how this immaterial cause . . . exists alike in the uterus and brain, or how the conceptions of the brain and uterus, answering to art and nature, resemble or differ from each other . . . I shall subsequently attempt to explain when I treat generally of the generation of animals . . . and when I speak of the soul and its affections, and how art, memory, and experience are to be regarded as the conceptions of the brain alone" (Harvey, 1651).

These writings have never come to light although as Paget (1850) pointed out, they could not have perished in the Civil War as Harvey made these references to them some years later.

Psychotherapy

Selden (1689) recorded Harvey's skill in dealing with patients suffering from mental illness: "A Person of Quality" complained that "he had four Devils in his head." Realizing "that 'twas only Melancholy that troubl'd him," Selden told the patient that apart from himself there was only "one Physician more in the whole Town that could Cure the Devils in the head, and that was Dr. *Harvey*." That his professional colleagues also appreciated his ability as a psychotherapist and turned to him when troubled in mind, we have on the authority of Sir George Ent who, in his preface to Harvey's *Exercitationes de generatione animalium* (1651), recounted how "Harassed with anxious, and in the end not much availing cares, about Christmas last, I sought to rid my spirit of the cloud that oppressed it, by a visit to that great man, the chief honour and ornament of our College, Dr. William Harvey."

Another patient, Bishop John Hacket, after going into "rural retirement . . . fell into a great fit of sickness. . . . Dr. Harvey, enjoined him two things—to renew his cheerful conversation, and take moderate walks for exercise, assuring him that . . . he observed more people die of grief of mind than of any other disease" (Plume, 1865). Indeed, he told him that "during the rebellion he had met with more diseases generated from the mind than from any other cause" (Buckle, 1872).

Of course Boyle's frequent references to his "Personal Maladies and Sickliness" and his inveterate medicine-taking reveal him as a hypochondriac from the days of his youth when he developed "a stuttering habitude," later "deep raving melancholy" (Boyle, 1744), and "a sensation of decay in my Eyes" (Boyle, 1693). His many affectionate references to Harvey may be taken as evidence of a good "transference" to him. Perhaps this was due to Harvey having relieved Boyle's anxiety about himself—in other words having helped him by psychotherapy.

Conclusion

Harvey's vivid lifelong interest in what are now neurology, psychiatry, and psychosomatic medicine is illustrated in his writings which foreshadow many fundamental advances. As these were

largely derived from clinical observation of patients it is perhaps not justifiable to maintain that "Harvey's clinical medicine did not differ appreciably from that of his contemporaries," (Bayon, 1938; 1939) although this may have been true of the common run of his practice. Furthermore, he had that appreciation of "the signal influence of the affections of the mind" (Harvey, 1649) which has always distinguished the great clinician.

ACKNOWLEDGMENTS

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The Philosophy of Circles–Cesalpino–Harvey

A Penultimate Assessment

WALTER PAGEL*

HARVEY'S discovery is rightly seen as the basis of modern physiology and scientific medicine. It should not be forgotten, however, that Harvey himself reflects the spiritual climate of a period of transition. He was a contemporary not only of Galileo and Santorio, but also of Van Helmont. In the latter we are aware of the intimate blending of scientific ideas and discoveries with spiritual symbols. In Harvey's case this is palpably less conspicuous. Yet with the idea of the "circle" Harvey has recourse to a non-scientific symbol. This was to give the scientific fact of the circuit of the blood its place in Aristotelian cosmology to which Harvey subscribed. It revealed that circular motion, the main driving force in the cosmos at large, was also operative in the living microcosm.

The application of analogies between macrocosm and microcosm to the motion of the heart and the blood had a long tradition. Just before Harvey, in 1590, Giordano Bruno summarised it in the statement that "in us blood continually and rapidly moves in a circle."[1]

I

The Idea of the Circle in Connection with the Motion of the Heart and the Blood prior to Bruno

a. *Plato*

It was Plato who gave the first clear formulation of the theory of microcosm, and it is he also who put forward the idea of a circular motion of the blood. He says:

> All the tissues as they are irrigated with it (*sc.* blood) repair their losses by evacuation. The character of this depletion and repair is the same as that of the movement in the universe whereby all things move towards their own kind. In fact, as the environment without us is ever wasting and dissipating us, scattering our various contents to their kind, so the contents of the blood,

* London, England.

1 *De rerum principiis et elementis et causis. Opp. latine conscripta.* F. Tocco and H. Vitelli, Eds. Florence, 1891, vol. III, pp. 507 *et seq.*, particularly p. 521. See the present writer in: Giordano Bruno, the philosophy of circles and the circular movement of the blood. *J. Hist. Med.*, 1951, *6*, 116-24.

in their turn, being broken up within our frame into tiny fragments and encompassed by the organism as by a heaven, are forced to imitate the motion of the universe; hence each of the fragments within us is carried to its likes and repairs their waste.[2]

In this passage metabolic products carried by the blood are presented in a motion imitating that of the universe and taking place in a body that is comparable to heaven. That the motion of the universe is circular and that there is a comparable motion of the blood ("αἵματος γοσά") we learn from other passages.[3]

These are not more than vague analogies designed to illustrate "sympathy" between cosmos and man on the one hand, and between like and like in the components of the body on the other. It would be unwise to see in them the proof of any presentiment or even knowledge of the blood circulation in Plato or the Hippocratic writers.[4] But they elaborate the microcosm theory with regard to the motion of the blood and thus become legitimate forerunners of Bruno's idea.

b. *St. Thomas Aquinas*

This is not so clear with regard to the idea of the circle in St. Thomas Aquinas' "opusculum" on the Motion of the Heart.[5] The author first analyses the relationship between the heart and the soul. To start with he rejects the possibility that individual faculties of the soul cause the movements of the heart: plants possess a nutritive, augmentative, and generative soul, but no heart. Nor are the sensitive or intellectual faculties responsible, since the heart moves involuntarily. Nor is it a "motus naturalis," as it con-

2 Plato. *Timaios,* 81A, B. A. E. Taylor, Tr. London, 1929, p. 86. See also: *Platon's Timaios oder die Schrift über die Natur.* Translated, with notes, by R. Kapferer and A. Fingerle. Stuttgart, 1952, p. 97, especially note 165. In this the loci from Plato and Hippocrates are collected from which the circular nature of movements called περιοδος, περιγοξα, γσζα emerges.

3 "The compass of the universe . . . is spherical and tends naturally to return into itself." *Timaios,* 58A, cap. 23; Taylor, *loc. cit.,* p. 58; see also 90D (cap. 43; Taylor, p. 97) "The flesh (a product of the blood) is loosened . . . falling itself back again into the circulating blood." (84B, cap. 39; Taylor, *loc. cit.,* p. 89).

4 See Diepgen, P. Haben die Hippokratiker den Blutkreislauf gekannt? *Klin. Wschr.,* 1937, *16,* 1820; Diller, H. Die Lehre vom Blutkreislauf, eine verschollene Entdeckung der Hippokratiker? *Arch. Gesch. Med.,* 1938, *31,* 201.

5 *De motu cordis ad magistrum Philippum. Opuscula Omnia.* Antwerp, 1612, XXXV, p. 214. In addition to this, the present writer used the edition Venetiis, 1490 (Hermann Lichtenstein Coloniensis) where the treatise occupies sig. J (verso) to J–2 (verso); it is a quarto volume in the possession of the writer. The first edition (a folio) was printed in 1488 at Milan by Beninus and Joh. Antonius de Honate. It was published again—separately with two other "opuscula"—in 1499 at Leipzig (personal communication to the author by Dr. H. A. Feisenberger). Dr. Lloyd Stevenson told the present author that he is also taking an interest in this treatise and is preparing a more detailed study through one of his pupils. Final assessment should therefore be deferred until its completion and publication.

sists of two components, that is, pulsation and traction. It is intrinsic and essential to the animal and therefore independent of anything outside the body, for example, a power in the universe such as that responsible for the tides. Nor, finally, is it due to heat which, on the contrary, it produces and which in itself fails to cause any motion.

The motion of the heart is intrinsic in the animal as a whole—just as the power to be attracted by a magnet is intrinsic in all iron. It moves *qua* iron, not because it is light or heavy. In the same way the heart moves, as its motion is immanent in the animal as an organism, in its "form." This may be compared to a state which keeps order by itself without needing a ruler. In this way the motion of the heart is linked with the soul, i.e., the "form" of the organism as a whole.

The more closely a particular "form" approaches functionally to a circle (the principle which underlies the motion of heaven), the higher its dignity. The motion of the heart is cyclic and therefore "circular"—hence it is the counterpart in animals to the moving heavens in the macrocosm.

By moving nearer and farther away a celestial body causes things to come into being and to perish, and by the continuity of its influence it preserves order in motion. The motion of the heart is the source of all other motions in the animal body. It is not perfectly circular like the celestial pattern which it imitates, but reaches high similarity with it by its cycle of pulsation and traction. Each cycle includes the beginning and the end.

St. Thomas Aquinas deals with the motion of the *heart,* not the *blood.*[6] He argues on the lines of the Aristotelian doctrine of the soul as the "perfection" (entelecheia) of the body as a whole. The motion of the heart cannot be explained in terms of action by any force outside the organism, or even any partial faculty of the soul. Like the latter it is intrinsic to the organism as such and, like the soul, reaches nobility and perfection by imitating the principle of celestial motion, *i.e.,* cycle and continuity. This consists in the rhythmic repetition of a beginning and of an end, namely, traction and pulsation. Bestowing celestial "perfection" on the animal, the circular motion of the heart is the most palpable demonstration of the theory of the microcosm.

[6] As suggested by Bayon, H. P. Wm. Harvey, physician and biologist. III. *Ann. Sci.,* 1938, *25,* 445.

The circular movement of the heart remained a topic generally discussed especially in the sixteenth century, notably by Leo Hebraeus, Cardano, and his adversary Scaliger.[7]

St. Thomas Aquinas' small treatise thus forms a further important source of the doctrine of the microcosmic cycle.[8] Because of the wide publicity which it enjoyed, this theory is likely to have come to Cesalpino's as well as Harvey's notice. That it did so, or even actually influenced them, remains a matter of speculation. The excellence of cyclic motion, its rôle as a link between microcosm and macrocosm, and its connection with the cardiovascular system are ideas common and of no mean significance to such Aristotelian naturalists as Cesalpino and Harvey.[9]

c. *"Pseudo-Allusions" and "Allusions"*
Cecco d'Ascoli—Lelio della Fonte—Davanzati Bostichi

There are a number of so-called "Allusions" to a circular movement of the blood before Harvey. None of these bears scrutiny. This particularly applies to the astrologer Cecco d'Ascoli and his famous didactic and encyclopaedic poem "L'Acerba."[10] Cecco was born in 1269 and burnt at the stake in 1327. In the XIth chapter we find a short anatomical survey in which some attention is paid to the "quiet movement" of the blood.[11] The passage which apparently contains the relevant "allusion" runs as follows:

[7] See the present author on Giordano Bruno (note 1).

[8] J. O. Leibowitz recently drew attention to the "circulatory" movement of the arteries occurring in the fourth Particula of Maimonides' aphorisms which deal with the pulse (*Korot*, 1955, *1*, 7-8). It is compared to the "moving of a ball, since the palpitation of the artery is explained to the senses by termination of the circuit." The present writer feels that this should be placed in proximity to St. Thomas Aquinas' idea on the cyclic motion of the heart as expressed in its systole and diastole, rather than the circulatory motion of the blood. This new perspective has been opened by Leibowitz' illuminating translation of the relevant term "g'mirath ha-sibbub"—"termination of the circuit"—which is a decisive advance over the traditional Latin version, "perfecta rotunditate" (personal communication to the writer by J. O. Leibowitz 9 April 1956). It is to be hoped that Leibowitz will resume and pursue this matter with reference to Galen as well as a possible influence on Aquinas.

[9] For detail see the present writer on Harvey and the purpose of circulation. *Isis*, 1951, *42*, 22-37; and Die Stellung Cesalpins und Harveys in der Entdeckung und Ideologie des Blutkreislaufs. *Arch. Gesch. Med.*, 1953, *37*, 319-28.

[10] See for biography and bibliography: Sarton, G. *Introduction to the history of science*. Baltimore, 1947, vol. III, pt. 1, pp. 643-5. Thorndike, Lynn. *History of magic and experimental science*. New York, 1923, vol. II, pp. 948-68. Neither Sarton nor Thorndike deal with his biological views and especially the above question. There are, however, two Italian papers, one on blood circulation according to Cecco d'Ascoli and Dante by Guiseppe Boffito (Publicazioni dell Collegio alle Querce di Firenze, 1934, 20 pp.) which the present writer was unable to consult, and the other by Adalberto Pazzini on medical doctrine in the Acerba (*Boll. Ist. stor. ital. Arte sanit.*, 1934, *14*, 230-49; 274-92) which comes to the conclusion that by motion of the blood Cecco meant a "movement de va et vient" but not circulation.

[11] "E sangue pian se moue con quiete."

Nasce dal cuore ciascuna artaria
Voglio che questi dicti in te reserui
E l artaria sempre doue vena
Artaria in se addoppia ogni via
Per l una al cuore lo sangue se mena
Per l altra uaccio lo spirito corre
Come splendore che moue de candela
Che senza tempo per l aiere descorre.

In this the following statements are of interest: (1) that the heart is the origin of the arteries, (2) that the arteries are everywhere accompanied by veins, and (3) that there are two sets of arteries, (a) one in which blood is led to the heart and (b) one that is "empty" and carries the spirit which is comparable to light in its splendour.

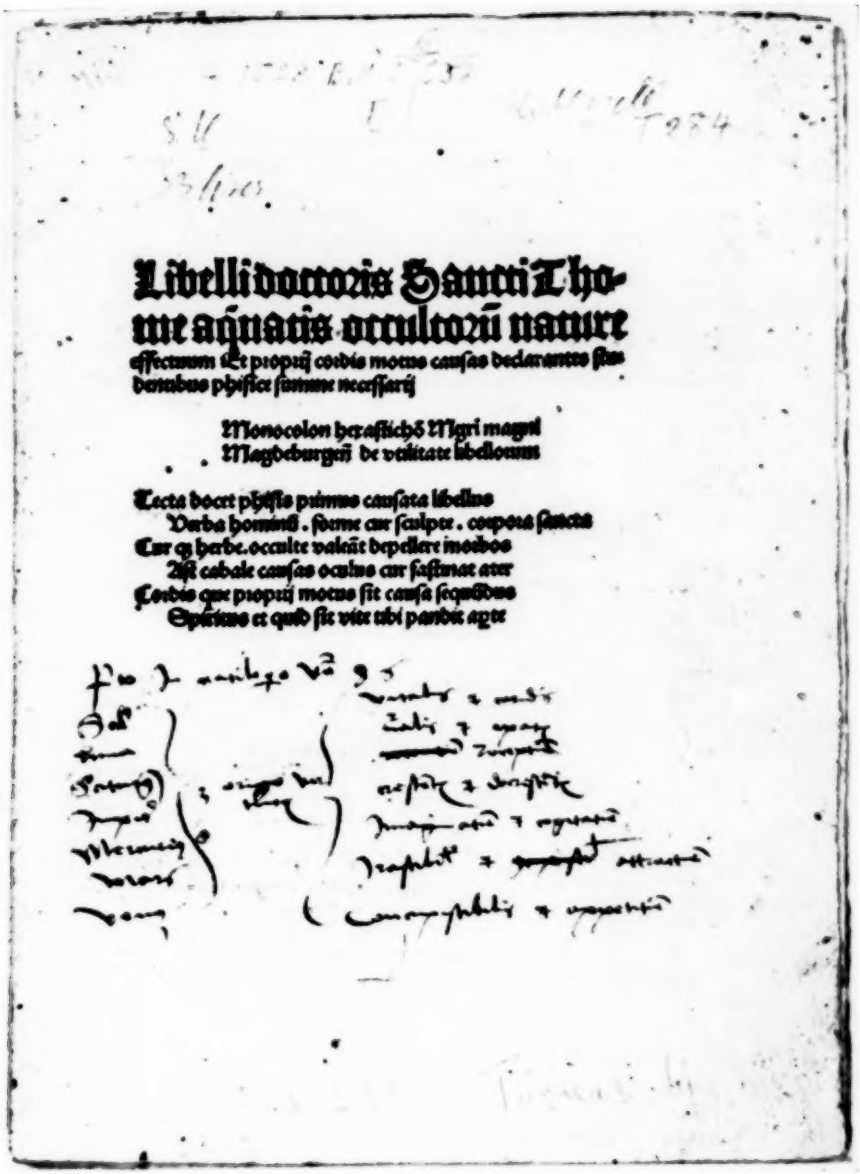
It is quite obvious that there is no question of "circulation" in this work. The veins are still believed to originate in the liver and probably supposed to take nutritional blood to the organs. That one set of vessels is believed to carry blood to the heart and another set spirit from the heart cannot be interpreted as a forerunner of the interpretation in terms of the centripetal venous and centrifugal arterial directions of the motion of the blood. For in this connection Cecco d'Ascoli talks of two sets of arteries without distinguishing between artery and vein. He also falls back to the ancient pre-Galenic opinion that some arteries are "empty" carriers of spirit.

The so-called "allusions" just dealt with and others are often met with in the promotional notes of antiquarian book catalogues, but not exclusively so. An example of such vague "allusions" occurring elsewhere is found in Antonio Francesco Bertini's (1658-1726) *Medicina difesa contro le calunnie degli uomini volgari e dalle opposizioni de dotti, divisa in due dialogi.*[12]

Bertini refers to the XXIst of the consultations of Lelio della Fonte.[13] In this the latter attributes refrigeration to the diastole of the heart, necessary because of its continual motion and also the flow of nutritive blood to the right heart so that it can be converted into vital blood. The systole, however, serves for the expulsion of sooty excrements and the attenuation of the blood in the right heart, its dispersion into the lung and through the aorta into all parts of the body respectively. In this way it is through the "cyclical

12 Lucca, 1699, pp. 76 *et seq.*

13 *De cordis palpitatione. Consultationes medicae in quibus vera vivaque consultandi effigies elucet.* Venice, 1608; edition used: Frankfurt, 1609, pp. 147-8. (By courtesy of Mr. L. M. Payne, Library of the Royal College of Physicians.)



Libelli doctoris Sancti Thome aquinatis occultorum nature effectuum Et propter cordis motus causas declarantes sue denabus physice summe necessarij

Monocolon heraclidis Magni Magdeburgen de veritate libellorum

Tecta docet physis primus causata libellus
Verba hominis . forme cur sculpte . corpora sancta
Cur q herbe occulte valeant depellere morbos
Astra cabale causas occultus cur sortitur ater
Cordis que propter motus sit causa sequentes
Spiritus et quid sit vite tibi pandit agite

FIGS. 1 and 2. Title-page and colophon (see verso) of the first and only separate edition of St. Thomas Aquinas, *De motu cordis*. Leipzig, 1499 (By courtesy of Dr. H. A. Feisenberger). It contains the text of the *De motu cordis* and not the treatise on the occult effects of nature (preceding *De motu cordis* in the "Opuscula").

ſſe vnmodo motus eſt voluntarius ſic hmodi motus quia eſt ſic impreſſi voluntarie Dicit aut philosophus i libro de anima ſa motus animalis q̄ maxime appareat aliquo non tamen habente intellectu mouel eq̄ et puerulus Et huius causam assignat quia necesse eſt alterari alteratione naturali aut media . alteratio aut partibus . hoc quidem augmti hoc aut decrementi pati vt iam moueatur et permutetur aut habiti permutatiõb⁹ inuicē Cum aūt mouēs alterio ſit calefactus et frigefactus que de ſolis et motus exiſtens naturalis et generationē vtiq̄ facti motus dicuntur partibꝫ ſi ē cordis et pulsi alteratõe incidente fiunt intellectus aut et phantaſia paſſionem afferunt vt concupiſcencie ire et huiuſmodi q̄ quibus cor calefcit vel infrigidat Et hec ē motus cordis dicus ſufficiat

Thelos

Impreſſum Lipcyg . per Jacobū Thanner Bambergenſem . Anno ſalutis noſtre .1.4.9.9.

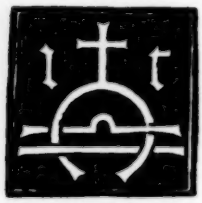


Fig. 2

Fig. 3. Early embryonic "disc" represented in a concentric circular pattern. From: Wilhelmi Langly *Observationes quaedam de generatione animalium* (1655) in: *Observationes et historiae . . . è Guiljelmi Harvei libello de generatione animalium excerptae . . .* Amsterdam, 1674. Tab. I, p. 137 (Photo by courtesy of The Wellcome Historical Medical Library).

52112

DISSERTATIO
ANATOMICO-MEDICA
DE
MOTU BILIS
CIRCULARI
EJUSQUE MORBIS.

Quam publicè olim habuit.

MAURITIUS van REVERHORST,
MEDIC. CAND. Lugd. Batav.
Nunc Professor Anatomicus
Hagæ-Comitis.

LUGDUNI BATAVORUM,
Apud JORD. LUCHTMANS, 1696.

FIG. 4. Title-page of Reverhorst *On the circular motion of the bile* (1692), separately printed 1696 (Photo by courtesy of The Wellcome Historical Medical Library).

movement" *("motum in orbem")* of diastole and systole that the nutritive blood is converted into vital blood and spirit. It is, therefore, the "cyclical" movement of the heart and not of the blood to which this author refers.

A little more of an "allusion" to "circulation" may be found in Bertini's reference to Bernardo Davanzati Bostichi's (1529-1606) book on money. In this, money is compared to blood—the juice and substance of food in the body which, running in minutes through the broad veins and watering the entire flesh, is made to evaporate by the heat of the latter as raindrops by the dry and hot earth. In the same way money—"the juice and substance of the earth"—runs through large purses in minutes, refreshes everybody and is spent and goes into the things that life requires, then returns into the same large purses and thus—circulating—maintains the life of the civic body.

We *conclude:* The symbol of the circle as connected with the cardiovascular function denotes either a "circular" movement of the blood or a "cyclical" movement of the heart. The former was first visualised by Plato and elaborated by Giordano Bruno, the latter was analysed on scholastic lines by St. Thomas Aquinas and formed a fixed topic for discussion by such sixteenth century writers as Leo Hebraeus, Cardano, and Scaliger.

II

Cesalpino

It is well known that Cesalpino concludes that there is a "perpetual motion of the blood from the veins into the heart and from the heart into the arteries."[14] It is equally well known that his insight in this respect seems to be marred by several passages in which Cesalpino refers to a movement of venous blood *to* the organs.

Cesalpino also described the movement of blood from arteries via the veins towards the heart.[15] It has been inferred that, here again, Cesalpino's insight was not real, but only appertained to certain special conditions such as ligature in venesection, sleep, and strangulation.

It is these two points which I propose to discuss briefly:

[14] Cesalpino, Andrea. *Quaestionum medicarum libri II.* In: *Quaestionum peripateticarum libri V.* Venice, 1593, lib. II, quaest. 17, fol. 234r.
[15] *Ibid.*

(1) Cesalpino says: "as streamlets receive their water from a source, the veins and arteries receive it from the heart."[16] "Veins and arteries go out from the heart."[17] "The vein must be continuous with the ventricles of the heart, as therefrom all virtue descends";[18] and even more startling: Aristotle stated that compression of the veins in the neck causes unconsciousness. In Cesalpino's opinion this effect is similar to that obtained by Galen through cutting of nerves. "For it is by the veins that a virtue of the heart is carried to the same places as by the arteries [i.e., to the brain], *since there is a communication between the openings of veins and arteries not only in the heart, but also throughout their whole course.*"[19]

This theme of the 'Peripatetic Questions' of 1571 is resumed in the 'Medical Questions' of 1593.[20] Here it says that on compression of the veins in the neck the virtue of the heart fails to be communicated to the brain whereby sensation and voluntary motion of the whole body are abolished.

Such passages and others which have been collected before[21] show that Cesalpino seems to adhere to the traditional postulate that nutritive blood is taken to the organs by the veins.

But how can we reconcile with this Cesalpino's correct ideas of the pathways of the blood? The answer to this may lie in the fact

[16] *Quaest. peripat.*, lib. V, quaest. 3, fol. 116A.
[17] Fol. 121D.
[18] Fol. 118E.
[19] Fol. 121D. The italics are of the present writer.
[20] Lib. II, quaest. 17, *loc. cit.*, p. 234.
[21] For example, by Willis, R.: *William Harvey. A history of the discovery of the circulation of the blood.* London, 1878, pp. 120 *et seq.* Sometimes Willis' interpretation seems open to criticism. See the present writer in *Arch. Gesch. Med.*, 1953, *37*, 322. This particularly applies to: *Quaest. peripat.*, lib. V, quaest. 3, fol. 116F (not B as quoted) and 117A discussed by Willis, *loc. cit.*, p. 121. In these passages the vena cava is said to originate in the liver and to send one large branch downwards for the nutrition of tributary parts, and also that "perfection of the blood" cannot be accomplished by the heart alone. But to Cesalpino it is the heart which does accomplish "perfection of the blood." Hence it is obvious that this passage does not give Cesalpino's own opinion, but a report of the Galenic theory as opposed to Aristotle. For the passage concludes: "Thus are the arguments whereby he (*sc.* Galen) militates against the opinions of Aristotle about the source of arteries and veins." (*Quaest. peripat., loc. cit.*, 117B.) It is however, Aristotle's opinions which Cesalpino makes his own, and the purpose of the whole work is their defence against Galen.

It must be admitted that Cesalpino is often particularly vague when talking about the anatomy of veins and arteries. An example are the *venae emulgentes* of the kidney. In his clinical work, published in 1606, they are said to enter the kidney "ex Vena cava et Aorta" (*Praxis universae artis medicae*, Treviso, 1606, p. 595). Obviously here Cesalpino makes no distinction between artery and vein. In a passage quoted by Peller (*Bull. Hist. Med.*, 1949, *23*, 218) from the *Catoptron* (ed. Frankfurt, 1605, VII, 1, pp. 488-9) the *venae emulgentes* are presented as branches of the vena cava tending towards the kidney into which they excrete superfluous water. This discrepancy shows that one cannot take isolated sentences from Cesalpino to prove one point or the other. Vagueness in anatomical data concerning the vascular system is probably due to Cesalpino's belief in unlimited anastomoses between arteries and veins throughout their whole course (see later).

that what appears to us as a contradiction need not have done so to Cesalpino. The latter was particularly intrigued by the widespread anastomoses between arteries and veins. He believed that the arteries were able to draw on the contents of the veins at any point in their course. Hence he might well have imagined that a centripetal flow of blood in the veins was compatible with some backflow of venous blood towards the organs through arterial anastomoses.

The passage quoted before[22] is difficult to interpret. Cesalpino defends *à tout prix* Aristotle's view of the heart as the centre of the Psyche. One of Aristotle's points was that compression of "veins"[23] causes unconsciousness—the same effect which Galen had achieved by cutting and squeezing of certain nerves. On the other hand, Galen had observed that an artery in the neck can be compressed without harm. Cesalpino concluded: Loss of consciousness on compression of the neck must therefore be due mainly to strangling of the vein. This seems to prove that Cesalpino believed in a centrifugal flow of venous blood towards the organ; but this proof may be merely apparent. To explain the effect of venous compression in the neck he expressly points to anastomoses.[24] He could have assumed that on compression of the neck parts of venous blood are missing which are normally drawn into the arteries through their venous anastomoses along their course and thus carried to the organs. For example, he says that "auctive" blood formed in the liver is carried by the veins to the arteries.[25] That the veins convey "something of the heart" may simply mean that blood originates in its entirety in the heart and that therefore also venous blood carries some cardial "virtue."

On the assumption that Cesalpino was carried away by his belief in the unlimited communication of venous and arterial blood through anastomoses, the juxtaposition of "contradictory" passages might be explained, for example in his work 'On Plants' of 1583. In this we find the general vague notion that "Veins distribute nourishment all over the body"[26] side by side with the statement that "we see that in animals nutriment is carried by the veins to the heart as the producer of innate heat, and, after acquiring ultimate perfection there, is distributed through the arteries

22 Fol. 121D.
23 As Cesalpino says himself, Aristotle did not distinguish veins and arteries properly.
24 Fol. 121D, as quoted before.
25 Fol. 119A.
26 *De plantis libri XVI*. Florence, 1583, cap. 1, p. 1: "Natura enim venarum, quae alimentum ex ventre hauriunt, ut illud in universum corpus distribuant, alique ex parte respondere videtur cum plantarum radicibus."

over the whole body by the action of the spirit which is generated from the same nutritive material in the heart."[27]

To *summarise:* The passages in which a centrifugal flow of blood in the veins seems to be expressed need not be contradictory to Cesalpino's assertion of the centripetal flow of venous blood. Such passages might have been based on his belief in an extensive exchange of venous and arterial blood through the anastomoses which he assumed to exist throughout the whole course of the vessels. As a result of this, venous blood might have been regarded as conveyed to the organs after entering an artery through anastomoses.

(2) The contention that Cesalpino's correct idea of the transition of arterial blood into veins appertained to exceptional conditions only was put forward by Tollin[28] and more recently by Peller[29] and was based on the famous passage of the 'Medical Questions' which we have mentioned before.[30] In explaining sleep and the waking state on peripatetic lines, Cesalpino says that while awake, spirit and blood are taken to the arteries and from there to the nerves, that in sleep, however, 'the heat' returns to the veins towards the heart and not through the arteries, for the natural way into the heart is by the vena cava and not by the artery. As Peller says, this is the only known passage in Cesalpino's entire works in which arterial blood is described as flowing back to the heart through the veins. However, the question remains whether Cesalpino really intended this backflow to be confined to the sleeping state—an exception to the general rule.

There is reason to doubt this contention. It is true Cesalpino says that in waking the movement of the native heat (i.e., blood) is outward to the sense organs and in sleep inward to the heart. But in this, emphasis is laid on a *quantitative modification of the blood flow*. Cesalpino says: In waking *much* spirit and blood, in sleep *less* "heat" are taken to the arteries. This is reflected in the pulse which is large, frequent, and vibrant in waking, but small, slow, and infrequent in sleep. For the arteries are distended *more* vehemently in waking, whereas the opposite is true of the veins. In

[27] *De plantis, loc. cit.*, cap. 2, p. 3: "Nam in animalibus videmus alimentum per venas duci ad cor tanquam ad officinam caloris insiti et adepta inibi ultima perfectione per arterias in universum corpus distribui agente spiritu, qui ex eodem alimento in corde gignitur."

[28] Tollin, H. Andreas Cesalpin. Bonn, 1884 (reprinted from *Arch. Ges. Phys. 35*, 387-90).

[29] Peller, S. Harvey's and Cesalpino's role in the history of medicine. *Bull. Hist. Med.*, 1949, *23*, 218-9.

[30] *Quaest. medic., loc. cit.*, lib. II, quaest. 17, fol. 234B.

other words, in waking there is also *some* venous backflow to the heart and in sleep *some* arterial outflow towards the sense organs, but it is palpably less than in sleep and in waking respectively.

A second point is that Cesalpino regards the contrast between sleep and waking as an event which makes the backward movement of arterial blood through the veins "manifest"—just as do the instances of venous ligature, and the compression of veins in the neck. These are, therefore, instances in which a certain process is evident, whereas, as one might infer, there are others in which it is also present, but not demonstrable. Cesalpino described the colour change of venous to arterial blood after prolonged venesection.[31] He was therefore aware of a further instance of the venous flow of arterial blood through anastomoses, although he made no use of it in the connection which is here at issue.

Thirdly, these apparently isolated instances are coupled with two general principles of Aristotelian doctrine: (a) that there is a perpetual movement from the vena cava through the heart and lungs into the arteria aorta—as had been explained in the 'Peripatetic Questions.' In this, Cesalpino confirmed the fundamental Aristotelian postulate that "when the external nutriment enters the parts fitted for its reception, the evaporation arising from it enters into the veins and there, undergoing a change, is converted into blood, and makes its way to their source (the heart)."[32]

(b) The Aristotelian doctrine that "evaporating matter is driven up to a point and then reversed like a tide race in a narrow street. For what is hot is driven in every animal upwards; when it has sojourned there, much returns and is carried downwards.[33]

To *summarise:* Venous backflow of arterial blood was probably *not* meant by Cesalpino to be an *exceptional* instance, but rather as a process that is made *manifest* by such instances as sleep and venous ligature.

We have perhaps spent more time on Cesalpinian exegesis than might be warranted. For a reconciliation of his "correct" with his "incorrect" statements hardly supports the extreme claims made

31 *Quaest. peripat., loc. cit.*, lib. II, quaest. 5, fol. 212C.

32 Aristotle. *De somno et vigilia*, 456^{b}3. In: *Parva naturalia*, T. I. Beare, Tr. Oxford, 1908.

33 "The matter evaporated must be driven onwards to a certain point, then turn back and change its current to and fro, like a tide-race in a narrow strait. Now, in every animal the hot naturally tends to move (and carry other things) upwards, but when it has reached the parts above (becoming cool) it turns back again and moves downwards in a mass." Aristotle (note 20), 456^{b}20.

on his behalf in the history of the discovery of closed blood circulation. Cesalpino did not discover it.

Our expository effort should show, however, that the interpretation of Cesalpino is not as obvious and easy as it may appear to admirer as well as to critic. But it was indeed Cesalpino who by following the Aristotelian lead collected data—interpretative as well as observational—which acquired acute significance in the discovery and were bound to contribute to it, consciously or unconsciously. His work and his views were far too famous to be overlooked by Harvey—himself a staunch Aristotelian.

WAS HARVEY ACQUAINTED WITH CESALPINO'S WORK?

We have no direct evidence proving Harvey's acquaintance with Cesalpino's work.[33a] Considering the circumstances, however, there can be little doubt about it. The 'Peripatetic Questions' went through two editions—the second appeared together with the 'Medical Questions' at Venice only five years before Harvey went to study 'next door' at Padua. Padua had been the stronghold of Aristotelianism for centuries. A restatement of its scientific and medical aspects confounding its adversaries and coming from the pen of the greatest living authority was bound to have wide publicity. This is borne out by Taurellus who took pains to refute Cesalpino's book point by point expressly in order to counteract its tremendous influence on the academic youth in Germany, notably at Altdorf.[34] Taurellus dealt in a somewhat confused way[35] with Cesalpino's views on the movement of the heart and blood and may not have been worth Harvey's attention.

This cannot be said, however, of the approval given to Cesalpino's views by Caspar Hofmann whose relevant writings, such as the authoritative commentary on Galen's 'Use of the Parts' (and

33a Mr. Frederick Kilgour kindly reminds me of the marginal scrawl on sheet 75 of the *Prelectiones anatomiae universalis*, London, 1886, which has been read: "J. Caesalpinus Aretinus." If this reading could be substantiated, it would be a reference to Cesalpino in Harvey's own hand, and no further argument were called for. However, this reading cannot be accepted, nor does it make sense in its context. As Dr. F. N. L. Poynter ingeniously discovered, it reads: "Julius Caesar Arantius" and refers to a passage actually occurring in Arantius' anatomical observations. For detail, Dr. Poynter's note attached to the present paper should be read. Internal evidence for Harvey's acquaintance with Cesalpino's work (as given above) is therefore still desirable.

34 Taurellus, Nicolaus. *Alpes caesae hoc est Andreae Cesalpini Itali monstrosa et superba dogmata discussa et excussa.* Frankfurt, 1597.

35 This is also Thorndike's opinion (*History of magic* [note 10], vol. VI, p. 331) whereas Tollin seems to take Taurellus more seriously (*loc. cit.*, pp. 310 *et seq.*).

probably 'On the Chest')[36] were known to Harvey. Hofmann was six years Harvey's senior and also a fellow student, though a little earlier, at Padua. Harvey quotes Hofmann's commentaries in the seventh chapter of *De motu cordis*, although he says he had written his own passage before reading them.[37] It is well known how Hofmann resisted Harvey's new doctrine in spite of the famous demonstration given to him personally by Harvey and its early acceptance by his pupil Slegel. Hofmann's resistance is surprising in view of his spirited and sustained rejection of the Galenic pores of the septum,[38] his assumption that arterial blood is driven into veins through anastomoses,[39] and his statement that in the embryo the heart forms two vessels—the artery and the vein—through which "the blood goes and returns."[40]

Hofmann refers to Cesalpino prominently in several places. He adopts his term "Circulatio" for the transference of blood from the right to the left ventricle through, and its refrigeration in, the lungs. In doing so he reveals that the term is meant in a "chemical" sense, denoting distillation. He says: "That the blood is transferred by *circulation*—to use the *term of the chemists*—I prove by the large size of the pulmonary artery which would be out of proportion if it were merely to nourish the lungs."[41] He supports Cesalpino's view that blood formed in the liver has no nutritional power and is not immediately taken to the organs, but acquires such power by arterialisation in the heart.[42] Hofmann agrees with Cesalpino that there is no need for Galen's "natural faculty" in explaining respiration. The cause is simply the expansion of the lung subsequent to the inflow of ebullient blood.[43]

He emphasises the heart as the source of all blood and all vessels, the communication between arteries and veins by anastomoses

36 Hofmann, Caspar. *Commentarii in Galeni de usu partium corporis humani libri XVII cum variis lectionibus.* Frankfurt a. M., 1625.—*De thorace e jusque partibus commentarius tripartitus.* Frankfurt, 1627.

37 With reference to Galen's insight into the reflux of blood made impossible by the valves: *De usu partium*, lib. IV. *De motu cordis*, 1628, p. 40; Willis, Tr., p. 44.

38 *De thorace*, lib. III, cap. 9, p. 94. Comment., p. 110-11.

39 Comment., II, cap. 10, p. 34.

40 Lib. XV, cap. 4, p. 337. "cor . . . non potest id [*sc.* fabricationem reliquarum partium] autem sine vasis, quae sunt illi instar aquae ductuum, per quos sanguis it reditque, statim facit sibi . . . venam et arteriam, quae sunt duo illa gemina, quae Coiter vidit primo die. . . ."

41 "Transferri sanguinem par circulationem talem ut cum Chymistis loquar, probat magnitudo . . ." Comment., p. 95. The italics are the present author's.

42 "Conficit quidem hepar omnem sanguinem, sed is nutrire non potest, nisi formam sanguinis adipiscatur. Adipiscitur autem admistione sanguinis arteriosi, qui in arteria magna it." P. 96 (lib. III, cap. 12) with reference to Cesalpino, *Quaest. peripat.*, lib. V, quaest. 4.

43 II, 16, pp. 42 *et seq.*

(which explains the virtual unity of all blood), and the continuity between portal vein and vena cava.[44] All these points had been made by Cesalpino. He also refers to the analogy between veins and plants as drawn by Cesalpino.[45]

There is no point in discussing further quotations from Cesalpino.[46]

Harvey may have made little use of Caspar Hofmann's works—he knew them well and cannot have failed to notice that Hofmann had drawn extensively and expressly from Cesalpino. Hofmann thus provides important circumstantial evidence for Harvey's acquaintance with Cesalpino's work.

Addendum by F. N. L. Poynter

In his note to Dr. Pagel, Mr. Kilgour was stating what has been generally accepted as fact, i.e., that Harvey mentioned Cesalpinus by name in his *Praelectiones*. The late H. P. Bayon, after long consideration of the passage which was thought to confirm this assertion, eventually published without qualification the statement that "in fact, Harvey mentioned Cesalpino in the *Prelectiones*." [*Annals of Science*, 1939, *4*, 367, lines 2-3.] The original error in the work of the committee of the Royal College of Physicians responsible for editing the transcript and facsimile of the *Praelectiones* in 1886 has misled all those who have since referred to this volume. That it is indeed an error can now be shown.

At the request of Dr. Pagel, I examined the reputed reference in the MS. and transcript of the *Praelectiones* (p. 75 verso, foot). The MS. has "J.Caes.Aratij / v.Arter.qut. ij d. / Art. venalis 4 dig. / admitt: unde Aditus / maior Aspera Art. /" This the editors have transcribed as "J. Caesalpinus Aretinus . . . [*etc.*]." Why 'J.' when the forename of Caesalpinus was 'Andreas'? Why Caesalpinus'? "Why not," I suggested to Dr. Pagel, "Julius Caesar Arantius?" His enthusiastic acceptance of this suggestion and his insistence that I should publish it as an addendum to his paper encouraged me to search in the published writings of Arantius for the passage to which Harvey was alluding. His celebrated book, *De humano foetu* (1564) must have been well known to Harvey, but although the last chapter discussed the vessels of the heart, the passage is not to be found there. But when the third edition of this book was published in 1587, two years before the death of Arantius, his anatomical observations were for the first time printed with it. Here the passage referred to by Harvey was located and the reference to Arantius (*not* Caesalpinus) definitely established.

The full title of this book is *De humano foetu liber. Tertio editus, ac recognitus. Eiusdem anatomicarum observationum liber: ac de tumoribus secundum locos affectos liber. Nunc primum editi.* Venetiis, Apud Iacobum Brechtanum, 1587. Chapter 33, beginning on page 92, is headed "Qua ratione sanguis in sinistrum cordis sinum, perveniat discepetur," and on pages 93-4 is found the following:

. . . vas enim illud [i.e. vena arterialis], duos digitos, in adulto corpore, facile admittens, tantum sanguinis, per systolem, in pulmones effundere porae videtur, quantum a cava, una diastole, sinus hauserit. Accedit arteriae venalis amplitudo, quae quatuor digitos in exortu qua parte connectitur excipiens nos facile in suspitionem deducere potest hanc ipsam, non solum aerem, ex aspera arteria: verum etiam sanguinem."

[44] II, 23, p. 56.

[45] II, 26, p. 63; p. 109 (anastomoses).

[46] Some further references to Cesalpino may be briefly quoted however: II, 19, p. 46 ("modulamur ab anima sentiente"); I, 8, p. 11 (Aristotle, according to Hofmann, observes but two ventricles; this is in no way negatived by Cesalpino's observation of three ventricles in fishes); III, 16, p. 98 (the firm substance of the heart is suitable to protect the "aetherea facula" as it is called by Cesalpino); p. 108 (rôle of the heart in initiating respiration).

Although 'stop press' research can never be entirely satisfactory, this basic fact has now been definitely established. The passage in which the reference occurs argues the presence of blood and not merely air in the 'arterial vein' much as does Harvey in his *De motu cordis*. Perhaps this hasty note will encourage another to compare the observations of Arantius with the writings of Harvey and elucidate the influence which the one had upon the other.

III

Harvey

But what has all this to do with Harvey? The answer is: Nothing—if by "Harvey" we understand the discovery of blood circulation and nothing else. The historian, however, will endeavour to present Harvey as a living personality in his own right, and a spiritual focus in the seventeenth century—a period which was to develop ancient and Renaissance tradition in its own way.

Seen in this light, the most arresting feature is Harvey's professed and often manifested adherence to Aristotle. In this respect he is strangely reminiscent of Cesalpino.[47]

It is true that Harvey had no hesitation in opposing Aristotle. An example of this is his assertion that not the heart, as Aristotle had taught, but the blood is the "seat" of the vital principle ("soul")—a doctrine which had been expressly rejected by Aristotle.[48] One might add that the decisive method by which circulation was established, namely, a computation, however simple, was alien to Aristotelian thinking. To us its employment by Harvey is sure of our acute interest and admiration. To Harvey it was one of a series of arguments[49] used in the search not only for a certain mechanism, but in particular for its special purpose. Harvey has been rightly called the "lifelong thinker" on the purpose of circulation[50] and indeed it is here where he is most palpably inspired by Aristotle.

The circle is the underlying symbol of the motion of the blood. But the use of this symbol goes much farther than this. Already in

[47] For detail see the present writer in *Isis* (note 9), p. 26.

[48] *De generatione*, II, Amsterdam, 1651, p. 301. Willis, Tr., London, 1847, p. 374.

[49] As Kilgour has shown, Harvey refrained from a full elaboration of the calculus with all the methods which were at his disposal. "Certainly, Harvey was not concerned with accurate measurement" (Kilgour, p. 417).—"Although he made measurements, albeit not precise ones, to prove the demonstration of the pulmonary transit, Harvey made no measurements to prove quantitatively the systemic transit" (p. 418).—"The reason why Harvey was no more precise in his measurements was that he did not have to be to demonstrate his great discovery" (p. 419).—"Harvey's elementary quantitation although adequate for his purposes and influential on a few biological investigators of the seventeenth century, had less effect than is often represented" (p. 421). Kilgour, F. G. William Harvey's use of the quantitative method. *Yale J. Biol. Med.*, 1954, *26*, 410-21.

[50] Curtis, J. G. Harvey's views on the use of the circulation of the blood. New York, 1915, p. 152.

De motu cordis, Harvey says: "Nature in death . . . retracing her steps, reverts to whence she has set out . . . and so animal generation proceeds from that which is not animal, entity from non-entity, so, by a retrograde course, entity, by corruption, is resolved into non-entity, whence that in animals which was last created, fails first; and that which was first, fails last."[51]

When he resumed this theme on a grand scale in *De generatione,* Harvey had been aware of the "circular" character of generation and its identity in principle with the motion of the blood for some thirty years. One is tempted to speculate whether Harvey might have been led to a special study of generation because its "circular" character might have promised to him some enlightenment in the vexed question as to the purpose of the circular motion of the blood. For it was this very purpose which defied him and seemed bound up with the purpose of "circular" processes in general. The answer is: Circular motion serves regeneration and preservation. Thus circular motion of the blood ensures the cyclical regeneration of its virtue in the heart after its consumption in the organs. In a similar way, generation is preservation and regeneration of the individual in the species.

This answer is based on the Platonic idea of Microcosm and on Aristotelian cosmology. In this, phenomena in the sublunary world are seen as imitations of the celestial pattern, notably the circular motion of the stars which, by virtue of its continuity, secures their eternal duration.[52] Nature has accomplished such a simulation of the rotatory motion of the celestial bodies by sublunary things "in the only way possible, namely by a cyclical arrangement of their serial phenomena."[53] This is manifested in the periodicity of such phenomena as gestation[54] and above all in the evolution of organisms. Here we see a circle formed by the sequence of germ, fetus,

[51] *De motu cordis.* Rotterdam, 1648, cap. IV, p. 55. Willis' tr., *loc. cit.,* p. 29.

[52] "The cause of this perpetuity of coming-to-be . . . is circular motion: for that is the only motion which is continuous. That, too, is why all the other things . . . imitate circular motion. For when water is transformed into Air, Air into Fire, and the Fire back into Water, we say that coming-to-be has completed the circle, because it reverts again to the beginning. . . ." Aristotle, *De generatione et corruptione,* II, 10; 337a, H. H. Joachim, Tr., Oxford, 1922. See also 338b: "The result . . . is concordant with the eternity of circular motion, i.e., the eternity of the revolution of the heavens."

[53] Aristotle. *On the parts of the animals,* W. Ogle, Tr. London, 1882, Lib. I, note 5, p. 143.

[54] *De generatione animalium,* IV, 10; 777a-b.

infant, man and the germ again, and so on. It is by this "cycle" that the individual is preserved in the species.[55]

The idea of the cycle also underlies the theory of "epigenesis" to which Harvey subscribed. Here again he is a true follower of the Aristotelian tradition. Epigenesis implies "creation"—the new production of parts in succession—as against preformation (or "metamorphosis") which consists of transformation of pre-existing material. Epigenesis is a process in which "nature or an operative faculty of a divine quality" are at work, not by supplying "fitting material" but by following a plan which insures the preservation of the species. "It requires and makes material which is . . . various in nature and variously distributed, and such as is now adapted to the formation of one part, now of another." All this comes, however, from "the same homogeneous material" and "makes out of a homogeneous material organs which are dissimilar." Thus out of "a similar material through the process of generation" the "dissimilar elements" emerge.

Preformation implies a once-and-for-all process which unwinds and must eventually come to an end. Epigenesis, on the other hand, is a cyclic process which can continue forever. The prime homogeneous matter represents the whole of the individual to be. It is there *before* any of the parts are formed. Here, then, is another example of the Aristotelian principle that "the Whole is earlier than the Parts." By contrast, in the bodies which develop through metamorphosis or preformation the Whole is gradually built up by the addition of part to part—the Whole succeeds their formation.[56]

The homogeneous Whole forms a circular "field." As such it is depicted in Langly's *Observationes*[57] which are meant to supply

[55] Aristotle says: "Since it is impossible that such a class of things as animals should be of an eternal nature, therefore that which comes into being is eternal in the only way possible. Now, it is impossible for it to be eternal as an individual (though of course the real essence of things is in the individual)—were it such it would be eternal—but it is possible for it as a species." *De generat.*, II, 1, 3; 731b. A. Platt, Tr. Oxford, 1910. And: "Since then no living thing is able to partake in what is eternal and divine by uninterrupted continuance . . . it tries to achieve that end in the only way possible to it . . . continues its existence in something like itself—not numerically, but specifically One." *De anima*, II, 4; 415b, 3-7, J. A. Smith, Tr. Oxford, 1931. Finally: ". . . in some sequences what recurs is *numerically* the same, in other sequences it is the same *only in the species.*" *De generat. et corrupt.*, II, 11; 338b, Joachim, H. H., Tr. Oxford, 1922.

Harvey himself refers, among other loci, to *De generat. et corrupt.*, II, 10: "The cause of the perpetuity [*sc.* of animal species] is, therefore, the law of the universe"—with reference to the cyclical approach and recession of the sun and their influence on generation and corruption. (*On generation*, I., Willis, Tr., p. 367.)

[56] *De generatione*, XLV, R. Willis, Tr., pp. 335-9.

[57] Langly, Wilh. *Observationes et historiae e Harveyi libello de generatione animalium excerptae.* Amsterdam, 1674, fig. 1, p. 137.

illustrative material to Harvey's book. It had been also described in this form before by Marcus Marci of Kronland.[58] The centre of the "field" is the seat of the driving generative force, the heart.

> The heart . . . construct (s) the rest of the body . . .; when erected it enters and conceals itself within its habitation, which it vivifies and governs. . . . And there it abides, the household divinity, first seat of the soul, prime receptacle of the innate heat, perennial centre of animal action; source and origin of all faculties; only solace in adversity.[59]

EPILOGUE

Harvey's death occurred at a time which saw the emergence of Iatrochemistry and Iatrophysics. Harvey, the Aristotelian, died in 1657, while Harvey, the discoverer and physiologist, was to remain immortal.

However, the symbolism of the circle lived on and found resonance even in sober observers and experimentalists of the iatrophysical school. Indeed, the conviction that everything in the cosmos moves "in a circle" led to interesting and scientifically correct observations and experiments concerning the movement of bodily fluids other than blood. It was thus that Reverhurst described in 1692 the continual "circular" excretion and reabsorption of bile.[60]

Finally, it was at this time—the second half of the seventeenth century—that belief in the supremacy of circular movement favoured the story of an anticipation of Harvey's discovery in the Hippocratic Corpus. One of its vociferous upholders is Raymond Restaurand (1627-82) from St. Esprit in Languedoc, Doctor of the Faculty at Montpellier[61] and author of several works advocating the resumption of Hippocratic physiology[62] and the Hippocratic branding iron in surgery.[63] In 1675 he had published: "Hippocrates on the circulatory movements in man from which emerges that the nature of the human body is circular and everything in it is moved in a circle." A later edition with an altered title in which

[58] *Idearum operatricium idea* 1635. For detail see the present writer: Religious motives in the medical biology of the XVIIth century. *Bull. Hist. Med.*, 1935, *3*, 224-31.

[59] Harvey, *De generat.*, XLVI, Willis, Tr., p. 341.

[60] Reverhorst, M. *Dissertatio anatomico-medica de motu bilis circulari ejusque morbis.* The Hague, 1692.

[61] Portal, M. *Histoire de l'anatomie et de la chirurgie.* Paris, 1770, tom. III, p. 605; Matthiae, G. *Conspectus historiae medicorum chronologicus.* Gottingen, 1761, p. 722 (§ 925).

[62] *Magnus Hippocrates Cous redivivus.* Lyons, 1681.

[63] *Hippocrates de inustionibus s. de fonticulis.* Lyons, 1681.

criticism of Harvey and other anatomists receives expression appeared in 1684.[64]

Summing up finally, we find ourselves to have moved in a circle. We set out with the Platonic idea of "Microcosm" and the imitation of the "circular" motion of the celestial bodies by the blood. In this connection we discussed St. Thomas Aquinas and other—false—"allusions" to blood circulation, before Giordano Bruno. We then attempted to disentangle ambiguities in Cesalpino's arguments concerning the movement of blood. Here we encountered the influence of Aristotelian doctrines closely related to the circle as a symbol. We next dwelt on Harvey's adherence to such doctrines as derived from Aristotle and his speculations on generation and epigenesis as examples of "circular" processes in the Microcosm additional to, and in some respects explanatory of, blood circulation. Finally, the survival of circle symbolism after Harvey brought us back to the ancient Greek theories with which we started.

[64] *Hippocrates de circulationibus humani corporis . . . naturam humani corporis circularem esse, omnia in eo moveri circulariter.* Lyons, 1675.—*Microscopium Hippocraticum sive judicium de umbris Harveyi, novae Medicinae principis, Pequeti, Villisii, procerum illius et de motu sanguinis in animalibus.* Ex typogr. regia Principis Elysiorum et industria Umbrae Elzevirii, 1684.—He had written in a critical vein before (1682), in a treatise on the consensus between the physiology of Aristotle and Epicure followed by a refutation of Gassendi, Descartes, and others: *L'accord des sentimens d'Aristote et d'Epicure, sur les principes des corps naturels et sur les couleurs. Avec la refutation des sentimens de Messieurs Gassendi, de Descartes, de Rohaut et de Cordemoy.* Lyons, 1682. 3 pts. The Harvey criticism is found in pt. II.

Sir Charles Scarburgh's Harveian Oration, 1662

L. M. PAYNE*

IT was on 24 July 1656, when William Harvey "had entertained all the Fellows [of the College of Physicians of London], together with some other friends to a magnificent banquet,"[1] that he formally handed over the deeds of his patrimonial estate of Burmarsh in Kent. In making this gift Harvey intended that there should be instituted an annual feast at which a Latin oration should be spoken to commemorate the benefactors of the College and exhort the Fellows "to study and search out the secrets of nature by way of experiment, and also, for the honour of the profession to continue in love and affection amongst themselves."[2] "The proceedings were brought to an end by an eloquent and vigorous speech by the President, also by Dr. Emily and by Dr. Scarburgh." Dr. Emily, regarded as the first Harveian Orator, seems unintentionally to have introduced a discordant note. Four days later, at an Extraordinary Comitia, he was accused of "having declaimed more bitterly than was proper against military matters, and also of having disparaged the present rule of the Commonwealth in a recent inaugural speech."[3] His Oration was reviewed by the Censors, and no further action was taken except to order that future Orators should submit the manuscript of their Oration to the President and some of the Censors at least one month before its delivery.

It seems strange that the list of Harveian Orators[4] contains the name of no one who is known to have been especially intimate with Harvey; one misses in particular the names of George Ent who persuaded Harvey to publish *De generatione animalium,* and Charles Scarburgh who succeeded Harvey as Lumleian Lecturer in 1656 and whom he remembered in his will, "I give my velvet gowne to

* Royal College of Physicians of London.

1 Royal College of Physicians. *Annals,* vol. 4, 1647-82, fol. 64a (MS.).

2 Munk, William. *Roll of the Royal College of Physicians of London.* London, 1878. vol. 3, p. 361.

3 See note 1.

4 See note 2.

my lovinge friend Mr. Dr. Scarburgh . . . and all my little silver instruments of surgerie." The list of Orators for the first ten years is:

1656, Edward Emily
1657, Edmund Wilson
1659, Daniel Whistler
1660, Thomas Coxe
1661, Sir Edward Greaves
1664, Nathan Paget
1665, Samuel Collins

Then comes a gap during which the College was pre-occupied with finding and building new premises to replace those in Amen Corner destroyed by the Fire of London in 1666. By the time the celebration was resumed in 1679 the Orators represent for the most part a new generation of Fellows who, unable to speak from first-hand acquaintance, could deliver only a commemorative address. Even the earliest Orators seem likewise to have limited themselves to formal utterances, knowing full well that their audience consisted of Fellows who had mixed freely with Harvey in the College and knew him as well as they did.

Four Orations belonging to this period are extant. The only one published is that of Sir Edward Greaves[5] and this contains no personal reminiscences of Harvey. Three others survive in manuscript. First, there is in the library of the Royal College of Physicians a copy of Edmund Wilson's Harveian Oration,[6] supposed to have been made by Sir George Ent. This is remembered, according to Munk and Lawrence,[7] because it contradicted the rumour then gaining credence with some persons that Harvey "to escape the pangs of dying had hastened his own end by an opiate." Wilson, in fact, makes no reference to this rumour, and merely gives an account of Harvey's death. The second is Christopher Terne's Harveian Oration in the British Museum;[8] it follows the pattern previously outlined, but it is undated. It contains flattering references to the Restoration of Charles II, and must, therefore, have been delivered after 1660. As Terne died in 1673, and since it will

[5] Greaves, Sir E. *Oratio Harveiana, 1661.* London, 1667.

[6] Ent, Sir George. *Apologia pro circuitione sanguinis . . . [and] Orationes Entianae.* 17th cent. (MS.).

[7] MUNK, *op. cit.*, vol. 1, p. 246; Harvey, W. *Opera omnia; [cum vita a T. Lawrence].* London, 1766. p.xxxiv.

[8] Sloane MSS. 1903.

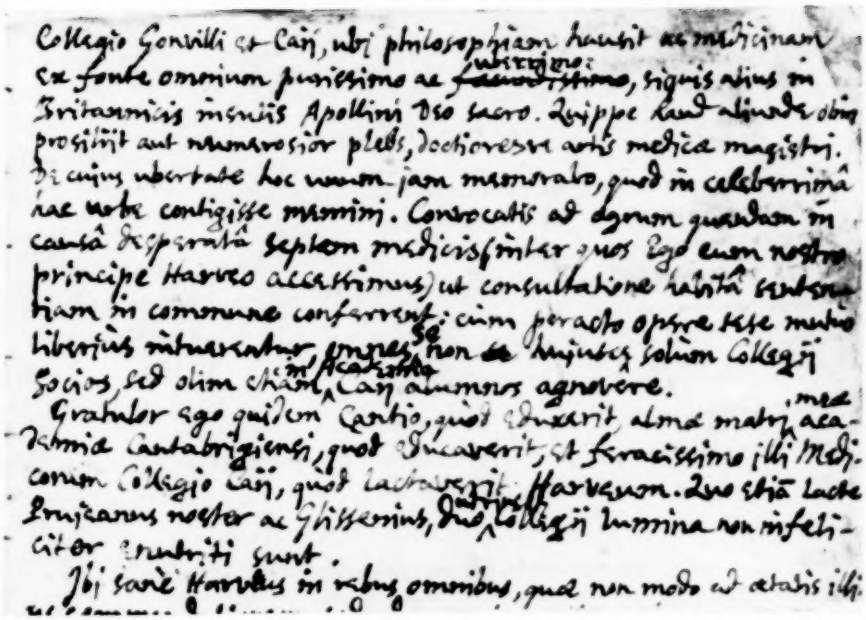
be shown that the Oration for 1662 is known, Terne's Oration must have been delivered on 23 July 1663. The *Annals* refer to the Harveian feast as having been given then but omit the name of the Orator.

Although Munk used the *Annals* of the College for his list of Orators he appears to have overlooked an entry dated 30 September 1661 appointing Dr. Scarburgh "Orator for the next month." It was at this period of the year that the Orator for the following year was appointed; and the Oration for 1661 had already been delivered by Sir Edward Greaves on the 25th of July. Since no other oration was then regularly delivered, it is difficult to see what this entry can refer to except to the appointment of Harveian Orator for 1662. "Month" must, therefore, have been written in error for "year."

The third manuscript[9] was discovered almost accidentally about a year ago, while searching for Harvey material. It was described as an anonymous Harveian Oration, c.1670—anonymous since the first of its nine leaves which would doubtless have given the name of the Orator is missing. A superficial examination of the text revealed a number of clues which, it was felt, would make it possible to identify the Orator beyond doubt; and even at this stage—before the Annals had been consulted—it was tempting to think that the Oration was by Sir Charles Scarburgh. Scarburgh was born in 1615, the year in which Harvey became Lumleian Lecturer, and was educated at St. Paul's School and Caius College, Cambridge, of which he became a Fellow in 1640. Soon after the outbreak of the Civil War his Royalist sympathies caused him to leave Cambridge and go to Oxford where he joined William Harvey, who was then Warden of Merton (1645-6). Scarburgh may well have met Harvey before this time through his uncle, Edmund Smith (1600?-54), himself a life-long friend of Harvey. Certain it is that from this time there sprang up a friendship as a result of their association at Merton. Scarburgh was created M.D. (Oxon.) in 1646, Harvey testifying that the candidate was well learned in physic, philosophy, and mathematics.

The Oration just referred to was certainly delivered between 1660 and 1665 for it contains references to the Restoration and its text shows that it was delivered in the same theatre in which

[9] Bodleian Library, Rawlinson MSS. D815.



Bodleian Library MS. Rawl. D815 fol. 3a in which Scarburgh speaks of a case to which he had been summoned with Harvey and five other doctors.

Harvey lectured. The author was a graduate of Cambridge for he recalls a case to which he was urgently summoned with Harvey and five other doctors, all of whom were found to have been "in Academia Cant. alumni." Scarburgh, as we have seen, was a graduate of Cambridge, but so were Coxe and Collins, while Paget, having taken his degree at Leyden, was incorporated M.D. at Cambridge in 1642. The Orator speaks with pride of his University and then goes on to refer to Caius College as "that college most fruitful of doctors," two of whom, Glisson and Prujean, were distinguished scholars of Caius College and Fellows of the College of Physicians. Can one read between the lines here and suggest that the author himself was a graduate of Caius? The author refers to Harvey as "meus praeceptor" and Scarburgh is the only one of those named who was a scholar of Caius College and taught by Harvey.

The evidence of the handwriting is not quite conclusive but assuming that the manuscript was written by the Orator (and the corrections made in the same hand lend support to that view) it cannot have been the work of Thomas Coxe or Samuel Collins. The former was Treasurer of the College, 1676-80, and the latter Registrar, 1682-85, and entries in the *Cash Books* and *Annals*, respectively, for these periods bear no resemblance to the hand in which the manuscript of the Oration was written. No specimen of Paget's hand, other than his signature, is known, but it can at least be said that Scarburgh's hand bears a closer resemblance to that employed in the manuscript than Paget's. The Oration is written in elegant Latin, and Scarburgh is known to have delivered an elegant Latin oration when the Marquess of Dorchester was admitted to Fellowship of the College in 1658.

On the evidence we have, the claims of Scarburgh to be considered the author of this manuscript are very strong. It is, however, unquestionably the work of someone who was intimate with Harvey, and these personal touches are perhaps of greater significance than knowledge of who wrote them.

What kind of picture of Harvey emerges then from the pages of this Harveian Oration? At first the Orator is confined by the demands of a commemorative speech and appears weighed down by the solemnity of the occasion and by the knowledge that the distinguished audience before him was so familiar with Harvey that it was difficult for him to say anything new. Soon, however, he begins to speak more intimately and although his language is still

at times exaggerated it is due less to the demands of a formal oration than to his desire to pay a fitting tribute to his friend and teacher. In recalling Harvey's life at Cambridge, he tells of

> the great goodwill, the esteem and the devotion he excited among those around him; for such was his nature. He devoted himself assiduously to his studies and turned with the greatest zeal to philosophy. After the manner of the ancient philosophers (as they say about Plato and Pythagoras) he thought that he should travel as widely as possible in the hope of acquiring thereby some of their teaching and wisdom. Therefore this genius, who was to become a second Aesculapius, did not continue to remain in this country.

He visited France, Germany, and Italy, and in particular the University of Padua. Here he became the disciple and later the friend of the celebrated Fabricius. It would seem that while he studied at Padua, and not before, Harvey began to think of his career.

> He thought again and again and for a long time how he could raise himself effectively from the ground and place his head among the stars and at last there settled in his mind the wish to embrace medicine. He practised this art with such skill that it seemed as though he had not acquired the knowledge but had rather been born with it. He took nature for his province and was bound by the laws of no single discipline. Without doubt there was nothing in nature encompassed in so great obscurity or so recondite that he would not immediately unfold an explanation with the utmost clarity and simplicity. To his genius and erudition were added a candour of mind, charm of manner and an affability which made it easy for all to meet the old man and ply him unceasingly with questions. One returned from his presence not only wiser—this was always so—but often more cheerful. The sharpness of his intellect continued unimpaired and a certain youthful disposition persisted to the end, as if this keen mind could not be weakened by age. The old man was just as eager to share his learning with those who sat at his feet as they were eager to seek it. His knowledge was not obtained for the sake of vanity or self-glorification, but was always available for the common good for which he would labour day and night.

As if to illustrate this quality the Orator recalls the reply Harvey made to his friends when they asked why he had not come to the aid of his little book on the motion of the blood, which several had attacked:

> It is [he said] of no great importance that I for my own gratification, should molest for a second time the republic of letters. I will not be the author or sponsor of any new controversial doctrine. Let my thoughts perish if they are worthless, my experiments if they are erroneous, or if I have not properly understood them. I am satisfied with my industry. It is not in my nature to upset the established order. If I am wrong (for after all I am but a man), let what I have written turn sour with neglect, but if I am right sometime at least the human race will not disdain the truth.

There is a somewhat ironical touch here since Harvey suffered the loss of many of his notes during the Civil War, although ideas outlast books and manuscripts.

It will be seen that the Orator has so far spent little time commemorating the other benefactors of the College. He may be said to achieve this in two ways. Towards the end of his speech he mentions the generosity of Baldwin Hamey and the Marquess of Dorchester in coming to the assistance of the College during a difficult period in its history. In another place, after referring to the debt this country owes to men like Gilbert, Bacon, Digby, and others, he speaks with pride of the contributions made to anatomical studies by Fellows of the College. He does not mention them by name, being content to suggest that nothing more would have remained to be discovered in this field, if the rest of Harvey's investigations had survived. In an eloquent condemnation of the havoc wrought by soldiers in the Civil War he notices among the papers that perished[10] those

> De Vegetatione ac Vita, De Respiratione vitae proxima; De Pulmone, De Cerebro, De Motu ac Sensu Animalium, De Insectis, and many other wonderful secrets of nature. He had studied the sea and land, islands and continents, mountains and valleys, woods and plains, rivers and lakes, and whatever mysteries they contained. He pursued most diligently the origin of all creatures, their food and habits.

In a passage the meaning of which is not quite clear the Orator recalls how Harvey often spoke of a project dear to his heart, which he had never fulfilled. It was the setting up of a professorship of experimental philosophy at Cambridge together with a laboratory and gardens furnished with every kind of plant, for the maintenance of which he would set aside an adequate sum; but the Civil War deterred him for

> he used to say with tears in his eyes "if I dedicated my property as I had determined to the promotion of the discovery of truth and to the public good I might just as well have made Anabaptists, fanatics and every kind of robber and parricide my heirs."

Of Harvey's personal habits he says,

> although it was not apparent to all, Harvey always cherished that philosophy which had a reverence for divine authority and uprightness of character so that he might regulate his life with integrity. When he perceived

[10] *Cf.* the list and comments published in Paget, Sir George, *Notice of an unpublished manuscript of Harvey*. London, 1850. p. 5 *et seq.*

that honours and other decorations of that kind tended rather to titles of honour than to that end, he passed all these things by. As you know, his tastes in dress and food, and in the other necessities of life were of the simplest; this simplicity was imposed on him by the demands of his personal philosophy.

The random recollections of the Orator serve to create a vivid portrait, so that we can sympathise with him as he exclaims

> Posteri sane nobis hanc felicitatem invidebunt,
> quod vidimus, quod audivimus Harveum.

ACKNOWLEDGMENT

My thanks are due to Sir Harold Boldero, the Registrar, for permission to quote from the *Annals* of the College and to Miss O. M. Lloyd and Miss M. F. Hellings, with whom I have discussed doubtful points in connection with the transcription and translation of the manuscript.

William Harvey's Last Will and Testament

The Official Probate Copy With the Grant of Administration Made to Eliab Harvey in May, 1659, Now in the Wellcome Historical Medical Library

F. N. L. POYNTER*

THE probate copy of Harvey's will reproduced here was purchased for the Wellcome Library in a London auction room in 1935. It has not so far been possible to trace its earlier history. It is written on vellum measuring 27½ by 32 inches folded three times across its width and twice vertically. On the outer fold is written in a seventeenth century hand the date 1659 and the name "Mr. Stone," and in later hands, "Docters Will" and "Doctor Harveys Will." The grant of administration has been attached to the foot of the will by a vellum tie in such a position as to obscure a portion of the text of the will and was temporarily removed for photographing. It is written on thin parchment measuring 4 by 6 inches in a much smaller and finer hand than the will itself.

The text of this copy corresponds closely to that published by Willis in the introduction to his translation of Harvey's works (London, Sydenham Society, 1847, pp. lxxxix-xcvi). The grant of administration is not the same as that published by Willis and bears different signatures, viz. Mark Cottle, R. Sankey, and Wm. Hobbs. Mark Cottle, who died in January, 1682, was Registrar of the Prerogative Office of Canterbury, which was the archbishop's court for dealing with questions of probate. Working in Cottle's office was the Richard Hoare who, in spite of having lost an eye through smallpox, was employed by Evelyn as amanuensis and whose "fine writing worke and flourishing" was seen and commented upon by Pepys (*Diary*, 26 Dec. 1665. See also *The diary of John Evelyn*, ed. de Beer. Oxford, Clarendon Press, 1955. ii, 559, n.2, iii, 421, n.2, and Musgrave, *Obituary prior to 1800*, London, Harleian Soc., 1900, ii, 79.).

It has not yet been possible to identify the R. Sankey and William Hobbs who also signed the grant, the transcript of which follows:

* Wellcome Historical Medical Library, London.

Richard Lord Protector of ye ComonWealth of England Scotland & Ireland and the Dominions & Territories thereto belonging To all persons to whome theise presents shall come Greeting Know yee yt uppon ye second day of May in the yeare of our Lord God one Thousand six hundred fifty Nine, before ye Judges for probate of Wills & granting Administrations lawfully authorized The Last Will & Testament of William Harvey late of ye parish of St Peters ye poores in London, but att Roehampton in Surrey Doctor in Phisicke Deceased was at London in Comon forme proved which Will is to theise presents annexed and Administration of all & singular ye goods chattells & debts of ye said deceased which any manner of way concerne him or his said Will was granted & Comitted To Eleab Harvey his brother the sole and only Executor named in ye said Will hee having first taken his oath well & truly to Administer ye said goods chattels & debts according to ye tenor & effect of ye same Will and to make or cause to be made a true & perfect Inventory of all & singuler ye goods chattels & debts of ye said deceased which have shall or may any way whatsoever come to his hands possession or knowledge and alsoe a true & just accompte in & Concerninge his said Administration when hee shalbe assigned or Lawfully called soe to doe which touching an Inventorie hee was presently assigned to performe at or before ye last day of June next Enseuinge Given at London under ye Seale of ye Court for probate of Wills & granting Administrations The day & yeare aforesayd

Mark Cottle Regr R. Sankey Wm.Hobbs.

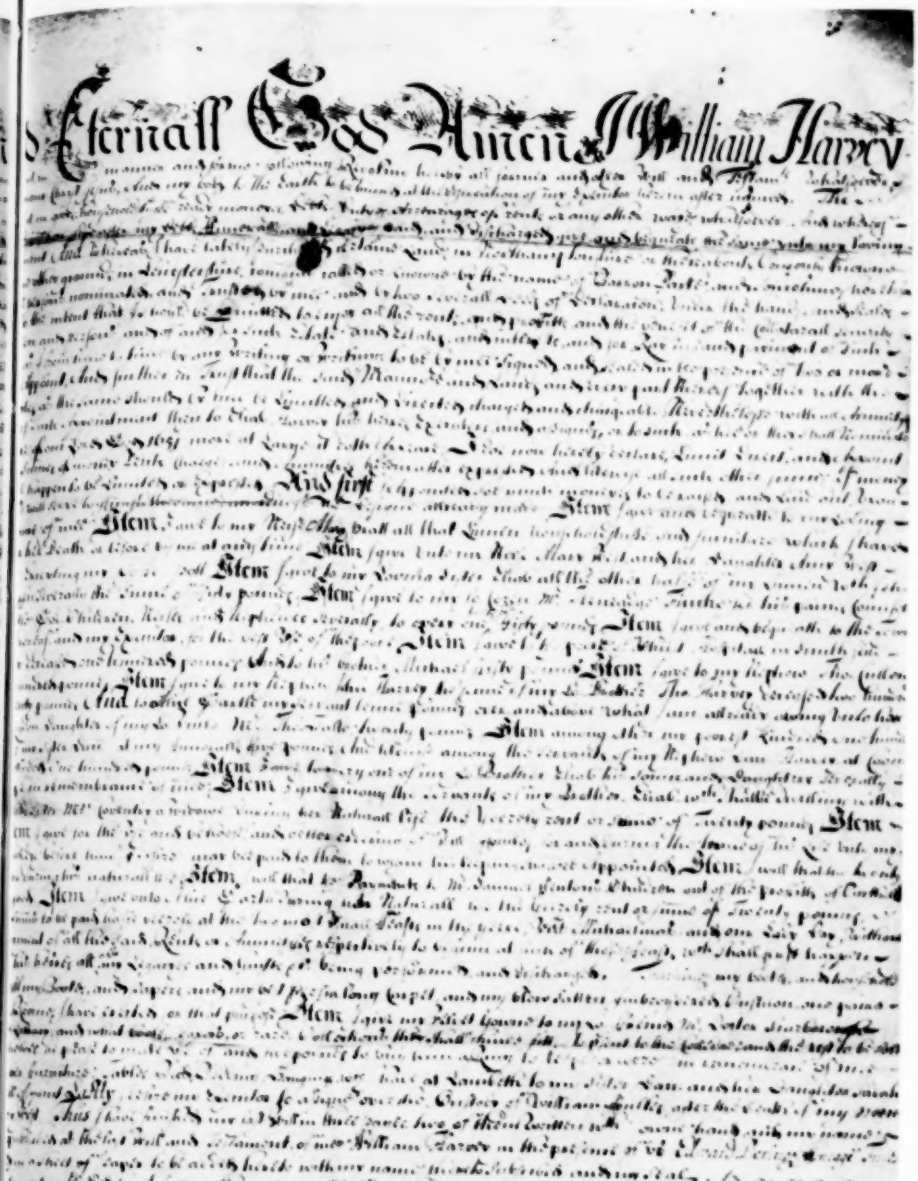
Richard Lord Protector of ye Comon Wealth of England Scotland & Ireland and the
Dominions & Territories thereto belonging To all persons to whome these p'sents shall come
Greeting Know ye that upon ye seaventh day of May in the yeare of our Lord
God one thousand six hundred sixty nine, before ye Judges for probate of
Wills & granting Admons lawfully authorized The last Will & Testam.t of William Harvey
late of ye parish of St Anne ye precinct London, but all Northampton in Surrey London Precinct Deced was at
Lambeth in Comon forme proved & Will is to this p'sent annexed and Admon of all & singular
ye goods Chattels & debts of ye s.d Deced wch any manner of way concerne Lease or title ye Will
was granted & Committed To Elizab Harvey his brother the sole and only exec.r
& Executor nominated in ye s.d Will &c having first taken his oath well & truly to Administer
ye s.d goods Chattels & debts according to ye tenor & effect of ye same Will and to make or cause to be
made a true & perfect Inventory of all & singular ye goods Chattels & debts of ye s.d Deced wch shall
or may any way hereafter come to &c hands possession or knowledge and also a true & just
accompt in Contermings &c ye s.d Admon when &c shall be assigned or lawfully called for
to doe wch touching an Instr &c was & p'sently assigned to performe at or before ye
last day of Term next Ensuing Given at London under ye Seale
of ye Court for probate of Wills & granting Admons The day & yeare aforesaid

Mark Pitt Regr

R Barley

Wm Hobbs

In the name of the Almighty an



The Echo of Harvey's *De Motu Cordis* (1628) 1628 to 1657

E. WEIL*

ONLY five years after Harvey's death Thomas Fuller wrote in his *History of the Worthies of England:* "The Doctor, though living a bachelor, may be said to have left a hopeful son to posterity: his book 'De Circulatione Sanguinis,' which I may call his son and heir: the doctor living to see it at full age and generally received." A good many years after the publication there was more opposition to the new discovery than approval. True, Robert Fludd was quick to recognize it; probably he knew more about the publication of Harvey's book at Frankfurt than anybody else—did he not in fact recommend William Fitzer as publisher?

The opposition set in at once. The second edition of *De motu cordis,* Parisanus' *Exercitationes* (Venice, 1635; Keynes 2), is an anti-Harveian publication, as is the reprint under the title *Recentiorum disceptationes* (Leyden, 1647) usually quoted as the sixth edition (Keynes 6); it was enlarged by the indefatigable Primrose who reprinted all the texts opposed to the idea of the circulation.

During the first ten years after the publication, only Francis Glisson and John Wallis had publicly defended Harvey. George Joyliffe (1627-58), M.D. Cantab., Thomas Wharton (1614-73), physician to St. Thomas's Hospital, Thomas Willis (1621-75), M.D. Oxon, Samuel Collins (1617-85) who studied at Leyden and took his M.D. at Cambridge in 1648, and the other Samuel Collins (1618-1710), the comparative anatomist—all the younger generation of learned doctors, even the famous Thomas Sydenham (1624-89)—remained mute as far as the great discovery was concerned. Besides Robert Fludd, Kenelm Digby (then an exile in Paris) was the first Englishman to write a favourable comment (1644). The tide, however, had turned with the publication of Descartes' *Discours de la Méthode* (1637). Most of the dates given are well known, and the 'cognoscendi' will find only a few new tiny silver bells tinkling with the praise of the great man.

. . . . . .

* London, England.

1629

FLUDD, Robert (1574-1637). *Medicina Catholica.* Frankfurt, William Fitzer, 1629. The theory of the circulation of the blood is definitely accepted. (See Pagel, W. Religious motives in medical biology. *Bull. Hist. Med.,* 1935, *3,* 277.)

1630

First printed adverse notice of Harvey's work

PRIMROSE, James (*c.* 1598-1659). *Exercitationes et animadversiones in librum G. Harveii de motu cordis. . . .* London, 1630. (Osler* 724.) In December 1629 Harvey had examined and passed Primrose as a licentiate of the College of Physicians.

c. 1630

COLLINS, John (1572-1634), Regius Professor of Physick, Cambridge, from 1626 to 1634, takes no notice of Harvey.

1631

First favourable notice in print

FLUDD, Robert. *Integrum morborum mysterium.* Frankfurt, 1631. (Page 11.)

1632

Second adverse notice

WORM, Ole (1588-1654). *Controversiarum medicarum exercitatio sexta.* Controversia V. Copenhagen, 1632. A good Galenist, Worm presents a series of careful arguments against Harvey's work. (See Gotfredsen, Edv. The reception of Harvey's doctrine in Denmark. *Acta med. scand.,* 1952, *142,* suppl. 266, 75-86.)

1634

READ, Alexander (*d.* 1641), anatomy lecturer, Barber Surgeons' Hall, from 1632. *A manuall of the anatomy of the body of man.* London, 1634. (See *The Library,* 1953, 5th ser. *8,* 201; this first edition is not in the STC.) Harvey is ignored; only in the 5th edition is notice taken of him. (See 1655.)

1635

PARISANO, Emelio (1567-1643). *De cordis et sanguinis motu. . . .* Venice, 1635. (Keynes 2.)

GASSENDI, Pierre (1592-1655). *De septo cordis pervio, observatio.* In: Severinus, Pinaeus. *Opusculum physiologum et anatomicum in duos libellos distinctum.* Leyden, 1650, p. 261. Gassendi's letter to Peiresc on Primrose's *Animadversiones,* 1630, was written 27 Oct. 1635 in defense of Harvey. (See Osler 726 and 6765.)

1636

HOFMANN, Caspar (1572-1648), a pupil of Fabricius ab Aquapendente at Padua, M.D. Basel, 1605, and professor of medicine at Altdorf, was not convinced of the circulation by Harvey's experiments. Harvey in a letter dated Nuremberg, 20 May 1636, offered to prove to Hofmann the validity of the circulation. (Willis, p. 216; Power, pp. 113-4.)

* For abbreviations of various bibliographical references, see end of paper.

[ENT, George.] *Laureae Apollinari.* Padua (1636). Collection of congratulatory verses addressed to Ent when he received his M.D. at Padua. First poem by Johannes Rhode of Copenhagen (*c.* 1587-1659; see Hirsch, *Aerzte Lexikon,* 1887, vol. 5, p. 6) refers to Harvey and the circulation.

1636-38

GLISSON, Francis (1597-1677), Regius Professor of Physick in Cambridge, read a dissertation on the circulation defended by John Wallis. Osler in his Harveian Oration of 1906 dated it in the "early thirties," but the date can only be 1636-38 when Glisson was there. See Bayon, p. 89. See also Wallis, 1641.

1637

DESCARTES, René (1596-1650) in his *Discours de la méthode pour bien conduire sa raison* (Leyden, 1637, p. 51) accepted the main principles of the circulation. He had read *De motu cordis in* 1632. (See Gilson, E. *Etudes de philosophie médiévale. Descartes, Harvey et la scolastique.* Strasbourg, 1921.)

VESLING, Johann (1598-1649). Letter to Harvey, criticizing Primrose and Parisanus. (Printed in *Observationes anatomicae et epistolae medicae ex schedis posthumis,* selectae et editae à Th. Bartholino. The Hague, 1640; reprinted Copenhagen, 1644.)

VESLING met Harvey during his Continental tour when attending the Duke of Lennox. His two letters of 1637 to Harvey are well known (see Willis, pp. 216ff.; see also Liceti, Fortunio. *De motu sanguinis: origine nervorum: . . . Quarto-quaesitis per epistolas clarorum virorum responsa.* Udine, 1647. Contains Vesling's "Responsio"). Liceto was an early opponent of Harvey (see next entry and Bayon, p. 93) but came round in time after long correspondence with famous physicians.

LICETI, Fortunio (1577-1657), disagrees with Harvey in a letter to Thomas Bartholin; printed in Bartholin's *Epistolarum medicinalium Centuria I & II,* Copenhagen, 1663.

BEVERWIJCK, Jan van (1594-1647), wrote two letters to Harvey from Dordrecht, dated 19 December 1637 and 16 September 1638. See his *Exercitatio in Hippocratis aphorismum de calculo. Accedunt ejusdem argumenti doctorum epistolae.* Leyden, 1641, pp. 190-9. There is also a letter from Harvey to Beverwijck dated London, 20 April 1638. These letters were not reprinted by Willis.

1639

DRAKE, Roger (1608-69). *Disputatio medica de circulatione naturali seu cordis et sanguinis motu circulari pro cl. Harveio.* Read at Leyden, Jan de Wale (Walaeus), Drake's professor, being President. The Thesis was printed the next year, Leyden, 1640.

PRIMROSE, James. *Animadversiones in Iohannis Walaei (Drake) disputationem quam pro circulatione sanguinis proposuit.* Amsterdam, 1639. (Reprint; see Keynes 3.)

BOË, Frans de le (SYLVIUS, Franciscus, 1614-72), gave full recognition to the circulation in his lectures in 1639. (Printed: *Disputationes medicae.* Amsterdam, 1663.)

FOLLI, Cecilio (1615-60). *Sanguinis à dextro in sinistrum cordis ventriculum defluentis facilis reperta via.* Venice, 1639. "The foramen ovale

question." Harvey deserved credit, he said, for reviving Galen's recognition of the circulation which had fallen into oblivion.

1640

GASSENDI, Pierre. *De septo cordis pervio observatio.* In: Pineau, Severin. *De virginitatis notis.* Leyden, 1640. "The foramen ovale question." Here Gassendi criticized Harvey adversely. (See Osler 726, 3680.)

REGIUS, Henry (ROY, Hendrijk du, 1598-1679), defended the thesis of Joa. Haymann: *Disputatio medico-physiologica pro sanguinis circulatione.* Utrecht, 1640.

PRIMROSE, James *Animadversiones in theses quas pro circulatione sanguinis Henr. Regius proposuit.* Leyden, 1640. (See Keynes 3.)

REGIUS, Henry. *Spongia qua eluuntur sordes animadversionum qua J. Primrosius. . . .* Utrecht, 1640. Reprinted Keynes 3. (See Osler 727.)

PRIMROSE, James. *Antidotum adversus spongiam venenatam Henrici Regii.* Leyden, 1640. Reprinted 1644. Sometimes found with Keynes 3. See Osler 725.

First German acknowledgment

CONRING, Hermann (1606-81). *De sanguinis generatione et motu naturali; opus novum.* Helmstadt, 1640. These eight dissertations were read by Conring while professor at Helmstadt; they were printed there in 1643. Conring was the first German physician to acknowledge Harvey. (See Osler 732.)

WORM, Ole (1588-1654). Letter of 11 May 1640 to T. Bartholin against Harvey, "An Pygmæi sind Dæmones." Printed in Bartholin, Epist. VI, p. 16.

1641

WALE, Jan de (WALAEUS, J., 1604-49). *Epistolae duae: de motu chyli et sanguinis ad Thomam Bartholinum,* dated Kalends of December 1640. In: Bartholin, Caspar, 1585-1629. *Institutiones anatomicae.* Editae a Thoma Bartholin. Leyden, 1641. He proved by experiments on animals the correctness of the Harveian doctrine. Often reprinted; see Osler 728-30.

DRAKE, Roger. *Vindiciae contra animadversiones D. D. Primrosii.* London, 1641. (Wing D 2132.)

ENT, Sir George (1604-89). *Apologia pro circulatione sanguinis, qua respondetur Aemilio Parisano, medico Veneto.* London, 1641. Reprinted, 1685. Osler 731. Wing E 3135.

WALLIS, John (1616-1703), states in a letter to Dr. Tyson that "Dr Glisson (who was then our Publick Professor of Physick, and under whom I kept, as they call it, a Physick Act) hath since told me, that I was the First of all his Sons, that did (in a publick Disputation) maintain Dr Harvey's Circulation of the Blood, in the year 1641" (*Phil. Trans.,* 1700-01, 22, 769). (See Glisson, 1636-38.)

1642

First acknowledgment in Italy

TRULLIUS, Giovanni (1598-1661), M.D. and physician to Pope Urban VIII (see Kaestner, *Medizinisches Gelehrten Lexikon,* p. 865) openly defended the circulation. See: Sinibaldi, G. B. *Geneanthropeia sive de hominis generatione.* Rome, 1642, Lib. V, Cap. XIV, co. 637. Sinibaldi's book was

reprinted at Frankfurt in 1669. (Willis, p. 253; Bayon, p. 95, quotes only the 1669 edition.)

BARTHOLIN, Thomas (1616-80). Letter of 14 July 1642 to Harvey, "Dubia quædam contra circulationem sanguinis." In Bartholin, Epist. XXIII, pp. 105-7.

1643

SEVERINO, Marco Aurelio (1580-1656). Letter dated Ides of April from Naples to T. Bartholin, "De Harveio judicium," in which Severino is critical of Harvey. In *Bartholin,* Epist. XXXI, pp. 127-8.

BARTHOLIN, Thomas. Letter of 8 May 1643 to Severino defending Harvey, "De pustulis lucentibus, & viperis." In *Bartholin,* Epist. XXXIV, pp. 132-6; see p. 136.

BARTHOLIN, Thomas. Letter of 18 June 1643 to Ole Worm, "De lucis origine et circulatione nova Liceti." In *Bartholin,* Epist. XXX, pp. 124-6.

WORM, Ole. Letter of 7 July 1643 to T. Bartholin, "De circulatione dubia. . . ." In *Bartholin,* Epist. XXXVIII, pp. 172-4.

LICETI, Fortunio. Long letter of 13 July 1643 to T. Bartholin, "De duplici sanguinis euripo." In *Bartholin,* Epist. XXXVII, pp. 149-72.

BARTHOLIN, Thomas. Letter of 13 August 1643, "Dubia de luce & circulatione soluta," answering Worm's letter of 7 July. In *Bartholin,* Epist. XXXIX, pp. 175-7.

1644

First favourable comment to be printed in English

DIGBY, Sir Kenelm (1603-65). *Two treatises. . . .* Paris, 1644. Pages 235, 238 are on the circulation. See Fulton, J. F. *Sir Kenelm Digby.* New York, 1937.

BEVERWIJCK, Jan van. *Epistolicae quaestiones, cum doctorum responsis.* Rotterdam, 1644. Letters of Descartes on the circulation, pp. 118-49. (Osler, 722, 723, 2030.)

PLEMP, Vopiscus Fortunatus (1601-71). *Fundamenta medicinae libri VI.* Louvain, 1644. On p. 126 Plemp acknowledged that Harvey was right; he was at first an opponent. (Willis, p. 254, quotes 1652 edition.)

Before 1645

SCULTETUS, Joannes (1595-1645), was a pupil of Fabricius ab Aquapendente at Padua and received his M.D. in 1621. I cannot verify D. F. Harris' quotation that Scultetus mentioned "Harvey has shown that blood to be the chief principle of the microcosm. . . ." (*Proc. XVIIth int. Congr. Med.,* 1913, hist. sect., 351-6.)

1645

LEICHNER, Eckard (1612-90). *De motu sanguinis exercitatio anti-Harveiana.* Arnstadt, 1645. Reprinted: Erfurt, 1650; Jena, 1653; Arnstadt, 1665. Leichner was professor of medicine in Erfurt and later town physician there. Bayon, p. 91.

1647

S., S. *Discours sceptique sur le passage du chyle, et sur le mouvement de coeur. Où sont touchées quelques difficultés sur les opinions des veines lactées, et de la circulation du sang.* Leyden, 1648. The little book is dated at the end from Leyden, 15 October 1647. Compare Osler 722 (3), 723.

1648

BACK, Jacobus de (*fl.* 1617). *Dissertatio de corde.* Rotterdam, 1648. See Osler, note to 735 and 695; Willis, p. 251, quoting Amsterdam 1649 edition. Translated into English, 1653: Osler 737.

RIOLAN, Jean, *fils* (1580-1657). *Encheiridium anatomicum.* Paris, 1648. To this Harvey replied with the *Exercitatio anatomica de circulatione sanguinis.* Cambridge, 1649 (Keynes 30, 31).

1649

First Spanish recognition

BRAVO de SOBREMONTE, Gaspar (1610-83). *Resolutionum et consultationum medicarum universam totius philosophiae doctrina.* Valladolid, 1649. (See Izquierdo, J. J. *Harvey, iniciator del metodo experimental.* Mexico, 1936, pp. 207 ff.; also Bayon, p. 100.)

RIOLAN, Jean, *fils. Opuscula anatomica nova. . . . Instauratio magna . . . de motu circulatorio sanguinis in corde.* London, 1649. (Osler 733. Wing R 1524.)

1649?

RIVIERE, Lazare (Riverius: 1589-1655), "publicly defended [at Montpellier] and taught the circulation of the blood, a candour for which he was by and by summoned by an adherent of the old school to resign his chair." (Willis, p. 251.) But where is it printed?

1650

SLEGEL, Paul Marquart (1605-83). *De sanguinis motu commentatio.* Hamburg, 1650. Slegel was a graduate of Padua. Harvey was well pleased with this treatise (see letter of March 1651; Willis edition of Harvey's *Works,* p. 596).

1651

HIGHMORE, Nathaniel (1613-85). *Corporis humani disquisitio anatomica.* The Hague, 1651. This is the first English anatomical work in which the circulation was fully recognized.

BARTHOLIN, Thomas. *Anatomia, ex Caspari Bartholini parentis institutionibus . . . tertium ad sanguinis circulationem reformata.* Leyden, 1651. (Willis, p. 253.)

PECQUET, Jean (1622-74). *Experimenta nova anatomica.* Paris, 1651. (Willis, p. 253.)

1652

MARCHETTI, Domenico (1626-88). *Anatomia: cui responsiones ad Riolanum . . . contra Veslingium additae sunt.* Padua, 1652. "Foramen ovale question." The mistake of Folli (1639) is corrected. (Willis, p. 256.)

RIOLAN, Jean, *fils. Opuscula anatomica, varia, et nova. Imprimis de motu sanguinis. . . .* Paris, 1652. (Osler 734. Willis, p. 249.)

RUDBECK, Olaf (1630-1702). *De circulatione sanguinis.* Uppsala, 1652. In support of Harvey. Bayon, p. 95.

POWER, Henry (1623-68). Manuscript notebook, "Circulatio sanguinis" (B.M., Sloane MS 1343). "Amongst all the rabble of his [Harvey's] antagonists, wee see not one that attempts to fight him at his own weapon, that is

by sensible and anatomical evictions to confute that, which he has by sense and autopsy so vigorously confirm'd." (Rolleston, p. 248.)

1653

ROLFINK, Werner (1599-1673). *Dissertatio de hepate ex veterum et recentiorum propriisque observationibus . . . et ad circulationem accomodata.* Jena, 1653. (Bayon, pp. 90-1.)

RIOLAN, Jean, *fils. Manuel anatomique et pathologique . . . de toute l'anatomie. Augmentée de la sixième partie, sur . . . la circulation du sang.* Paris, 1653. Pages 696-738: Riolan's last word in his dispute with Harvey.

1654

WEBSTER, John (clerk in Holy Orders). *Academiarum examen. . . .* London, 1654. (Wing W 1209.) Page 74: ". . . our never sufficiently honoured Countryman Doctor Harvey, discovered that wonderful secret of the bloods circulary motion."

1655

BARTHOLIN, Thomas. *Defensio vasorum lacteorum . . . Accedit cl. v. Guilielmi Harvei de venis lacteis sententia expensa ab eodem Th. Bartholino.* Copenhagen, 1655. Page 179: Bartholin praises Harvey for the discovery of the circulation, but he neglected "chyliferarum venarum pretium."

HARVEY's letters to Dr. J. D. Horst, physician at the Court of Hesse-Darmstadt, of 1 February and 13 July 1655. Printed in Horst's *Manuductio ad medicinam.* Ulm, 1660, pp. 59-82. Reprinted by Willis in Harvey's *Works,* pp. 612-15.

NARDI, Giovanni (*fl.* 1634). *Noctes geniales, annus primus.* Bologna, 1655. (Published also with dates of 1656 and 1657.) Pages 712-46: discussion of Harvey's discovery.

PEIRESC, Nicolas Claude Fabri de (1580-1637). Gassendi, Pierre. Viri illustris *Nicolai Cl. Fabricij de Peiresc . . . vita.* Hague, 1655. For passages referring to Harvey and the circulation, see Osler 6764-5. Translated into English, 1657.

READ, Alexander. *Manual of anatomy.* London, 1655. This, the fifth edition, is the first in which notice of Harvey is taken. Read was one of the seven surgeons who in the presence of Harvey examined the four Lancashire witches on 2 July 1634.

c. 1655

WINSTON, Thomas (1575-1655). *Anatomy lectures at Graham Colledge.* London, 1659. (Wing W 3078.) Winston mentions Harvey, but not the circulation; he was a censor of the College of Physicians and had studied at Padua. He "must have been present at Harvey's demonstrations" (Rolleston, p. 250; Bayon, p. 83).

1656

COLLOP, John (1630-60). *Poesis rediviva: or Poesie reviv'd.* London, 1656. See Poynter, F. N. L. An unnoticed contemporary English poem in praise of Harvey and its author, John Collop, M.D. *J. Hist. Med.,* 1956, *11,* 374-83.

ROLFINK, Werner. *Dissertationes anatomicae.* Nuremberg, 1656. Books V and VI. (Bayon, p. 91.)

1657

Inscription on Harvey's tombstone on the West wall of the North transept of the church at Hempstead, Essex

(Translation from the Latin ascribed to Sir George Ent)

William Harvey, at the mention of whose honourable name all academies rise up out of respect, who was the first after many thousand years to discover the daily movement of the blood, and so brought health to the world and immortality to himself, who was the only one to free from false philosophy the origin and generation of animals, to whom the human race owes its acquirements of knowledge, to whom medicine owes its very existence.

ABBREVIATIONS

Bartholin. BARTHOLIN, Thomas. *Epistolarum medicinalium Centuria I & II.* Copenhagen, 1663. (All references are in Centuria I.)

Bayon. BAYON, H. P. William Harvey, physician and biologist: his precursors, opponents and successort. Part I. *Ann. Sci.*, 1938, *3*, 59-118.

Keynes. KEYNES, Sir Geoffrey. *A bibliography of the writings of Dr William Harvey 1578-1657.* 2d ed. Cambridge, 1953.

Osler. OSLER, *Sir* William, *Bt.* Bibliotheca Osleriana. Oxford, 1929.

Power. POWER, *Sir* D'Arcy. *William Harvey.* New York, 1897.

Rolleston. ROLLESTON, Sir Humphry. The reception of Harvey's doctrine of the circulation. In: *Essays on the history of medicine presented to Karl Sudhoff.* C. Singer and H. E. Sigerist, Eds. London, 1924, pp. 247-54.

Willis. WILLIS, R. *William Harvey. A history of the discovery of the circulation of the blood.* London, 1878.

Wing. WING, D. G. *Short-title catalogue of books printed in England . . .* 1641-1700. New York, 1945-51. 3 vols.

On William Harvey at Padua and the Way in Which He was Stimulated to Reinvestigate the Problem of Heart and Blood Movements and on the Credit He Merits for the Discovery

L. CHAUVOIS*

WISHING to contribute to the homage paid by the *Journal of the History of Medicine* to an illustrious discoverer on the tercentenary of his death, I should like to attempt a just appraisal of the influences which turned him during his student years at Padua toward the mysterious problem of the movement of the heart and blood, supporting my discussion by my translations of the Latin works reputed to have preceded him. I must begin by saying, however, that these influences, contrary to what has been claimed, were far from providing him with a reliable demonstration of this "circular circuit" which he was to explain and demonstrate so magnificently.

Stimulated by national loyalties and the desire to minimize the merits of the Great Discoverer, some have said here and there that Harvey had only to bend down in Padua to "gather up" a "circulation" already made. In opposition, I wish to emphasize the colossal effort he had to exert either to confirm or invalidate the first timid gestures of opposition to the Galenic doctrine made by a certain few members of the avant garde not totally blinded by "the religion of the Master."

It was undoubtedly at Padua that Harvey, hearing certain contradictory echoes, first became suspicious of the perfect orthodoxy of the teachings. But if it is true, as has been claimed, that the hypothesis of circulation had already been so clearly posed and demonstrated, how could it be possible that the flagrant errors of Galen were still taught at that time—and for yet another century—in all the universities of the world? The fact is that the entire task of demonstration remained to be done. What had been advanced by Colombo (1559) who, furthermore, was undoubtedly familiar with the "transpulmonary" passage of Michael Servetus (1553), and by Andrea Cesalpino (1583), with his unquestionable intuitive

* Paris, France.

knowledge of the "general circulation" (as proven by the long passages I have quoted in a book which will be published shortly),[1] was no more than feebly established. Entirely lacking was the admirable series of observations, experiments, and controls that Harvey was to carry on for almost thirty years before daring to write about them. It was as if he himself were stupefied before the prospect of overthrowing the Galenic theory which had reigned for almost fifteen centuries. If one compares William Harvey's

. . . . monumentum aere perennius,
Regalique situ pyramidum altius,[2]

to the colorless, incomplete descriptions in the works of his predecessors, it is like seeing a magnificent, elegantly decorated palace beside a miserable shanty! The contrast is as striking as if one tried to compare the Palace of Versailles with the primitive hut of a woodcutter on the pretext that both are indisputably dwellings. To emphasize this important comparison, I have reproduced in my *Vie de William Harvey* the famous old texts which have been invoked to discredit him. They are certainly of infinite interest, but in my opinion, they only serve better to emphasize the mastery of the great English physician.

Let us take, for example, Andrea Cesalpino who, among the contemporaries of the young student at Padua, was the anatomist who strayed farthest from the Aristotelian and Galenic rut represented by the teaching of the day. I point out the following in my book:

(1) The young William Harvey could not have been ignorant of the existence of Cesalpino and his works, considering that the celebrated anatomist who had taught for a long time in Pisa still lived and even taught in Rome. And the works of said Cesalpino, namely, *Quaestionum peripateticarum libri V* and *Quaestionum medicarum libri II,* which had just been republished in Venice in 1595 by the famous Giunta printers, could not have been unknown to anyone in Padua, only a few leagues from Venice;

(2) In these works (and I give the Latin texts which deal with the circuit of the blood), Cesalpino, his ideas based particularly on the disposition of the cardiac valves, suggests clearly and at some length the probability of a complete, one-way circuit. And he did

1 *Vie de William Harvey.* First publication in May 1957 in two editions, English (Hutchinson, London) and French (Editions Sedes, Paris).

2 Horace, *Odes,* III, 24.

not limit himself (as has been claimed by certain individuals who still make assertions without any reference to the original texts) to several scattered, vague, and aberrant phrases which they claim to be so artfully arranged as to put words into an author's mouth! It is a sorry commentator who repeats fourth-hand that which others, just as poorly informed, have themselves "copied and recopied." Therefore, I felt obliged to quote at length the texts of Cesalpino in my work.

(3) Nevertheless, his hypothesis and his intuitive ideas lack substantial physiological demonstrations, and the latter are definitely linked to Cesalpino's speculation on a circular circuit which he based on the observation that the arteries and veins change in appearance in sleep and in wakefulness.

The following is my translation of the Latin text in the *Quaestionum medicarum libri II,* to which I have just referred:

> . . . As I have explained in the *Quaestionum peripateticarum libri V,* the disposition of the valves in the orifices of the vessels results in a perpetual displacement from the vena cava through the heart and lungs into the Aorta. In wakefulness, there is a movement of native heat carrying it toward the exterior, i.e., towards the sense organs; and in sleep there is an opposite movement toward the interior, i.e, towards the heart. Thus, one must conclude that in the former state much spirit and blood *enter the arteries,* since it is by them that access is had to the nerves; and that in the latter state the heat mentioned returns to the heart through the veins—and not by the arteries, since nature's chosen route to the heart is the vena cava rather than the aorta. Witness the differences in pulsations: while the individual is awake, they are strong, rapid, and vibrant; while asleep, however, they are slow and weak. The fact is that the native heat introduces itself into the arteries to a greater degree in wakefulness than in sleep. The reverse obtains in the veins which are more pronounced in sleep than in wakefulness . . .

To this is added, it is true, the observation by Cesalpino of the effects of the tourniquet which obstructs the venous "return" circulation:

> Just as the flux of blood toward the peripheral parts and its reflex toward the deep parts by means of this (arterial) outflow and (venous) return is thus manifested in sleep and wakefulness, in the same way may be explained the displacement of blood when constriction or any other means of closing the veins is applied to some part of the body. When the flow is effectively suppressed in this way, the small vessels in this region, which had previously shown normal circulation, are seen to swell; in the meantime, the blood on the other side moves towards the heart to avoid interruption of flow.

However, I repeat that the sum of Cesalpino's contribution to the concept of a continuous one-way blood circuit, first formulated

by him on the basis of the anatomical disposition of auricular and ventricular valves, does not exceed these two simple physiological observations. And how many answers and conflicting observations were offered in opposition by the partisans of Galen! It was indeed a discouraging task—that of trying to change even part of such a well-defended system—absurd as it may appear to us today! But Harvey is to come, Harvey who anticipates all these objections, overcomes them all, permitting no fraction of them to pass, and succeeds in constructing this imperishable monument of logic on the solid foundation of experiment and observation.

By way of brief outline, I wish to enumerate here the headings of the seventeen chapters of the *Exercitatio anatomica de motu cordis et sanguinis* (1628) although this simple reminder be far from rendering apparent the wealth of ingenuity hidden beneath such headings.

Chap. I—The author's reasons for dealing with the subject.

Chap. II—The movement of the heart as seen in animal experiments.

Chap. III—The movement of the arteries as seen in animal experiments.

Chap. IV—The movement of the heart and auricles as seen in animal experiments.

Chap. V—Mode of action and function of the movement of the heart.

Chap. VI—The routes by which the blood is carried from the vena cava to the arteries or from the right to the left ventricle of the heart.

Chap. VII—The blood travels from the right ventricle of the heart, through the lungs, into the vein-like artery and the left ventricle.

Chap. VIII—Of the quantity of blood which passes through the heart, moving from the veins to the arteries, and of the circular movement of the blood.

Chap. IX—The circular route of the blood is made evident by this first verified hypothesis.

Chap. X—The conclusion of a circulation of the blood, based on the amount which traverses the heart, is freed from objections and then proven by experimentation.

Chap. XI—The second hypothesis is confirmed.

Chap. XII—The existence of a circular blood route is further emphasized by the confirmation of this second hypothesis.

Chap. XIII—The third hypothesis which in turn demonstrates the existence of a circuit of the blood is confirmed.

Chap. XIV—Conclusion to the demonstration of the circulation of blood.

Chap. XV—The circulation of the blood is indicated by still other plausible reasons.

Chap. XVI—The circulation of the blood proven by its consequences.
Chap. XVII—The circular movement of the blood established by convincing evidence found in the heart and shown strikingly in dissections.

By contrasting the very first arguments advanced against Galen by the precursors, who were incontestably Colombo (repeating Servetus) and Andrea Cesalpino, with the work of Harvey, nourished by years and years of research, observation, experiments, and controls, one will be able to understand why Harvey did not mention Cesalpino (any more than his contemporaries Sarpi or Eustachio, all of whom had proven so little of their anti-Galenic arguments): to do so would have been to consider as solved a completely unsolved problem. He spoke only of Colombo whose critical affirmation (subsequent to that of Servetus, of whom Harvey seemed ignorant) of the nonperforation of the "septum interventriculare" appeared to Harvey to be *the principal point* of departure for all the research that remained to be done. This I believe to be the impartial judgment which should rightfully be made.

At this point, if I were not limited by space, I should attempt to destroy as well the legends of the circulation of blood said to have been mentioned since the end of the fifteenth century and the beginning of the sixteenth in the writings of Leonardo da Vinci (according to some) and in those of Rabelais (as others would have it). In spite of my profound admiration for their great memories—and enough glory has been heaped upon them so that they have no need of that belonging to others—I explain in my book the errors of their over-zealous partisans. For example, the writings and drawings of Leonardo bear witness to his belief in the interventricular "perforated septum" of Galen. This in itself indicates his attachment to the old, classical doctrine of his time, the doctrine which Harvey was to invalidate only 150 years later.

As to Rabelais, in my book *Circulation du sang: Schèma nouveau*,[3] published in 1933, I replied to certain over-imaginative commentators that Rabelais was so far from having thought of a "circular circuit" of the blood that I could use his description of Pantagruel's blood in the Third Book as a *model for an account of the state of traditional Galenic ideas* at the dawn of the Renais-

[3] Preface by Dr. Charles Laubry, Member of the Institute and of the National Academy of Medicine.

sance. Rabelais' marvelous transcription of Galen is not precisely a heralding of the arrival of Harvey!

I hope I have displayed in my account all the objectivity and independence it is fitting to exercise in discussing the genesis of great discoveries when trying to enumerate the events which always precede them but whose importance is sometimes not understood. They are usually destined to remain stagnant and sterile for a long time, sometimes even to disappear until the advent of a man of genius who, searching straight to the roots of the problem, never leaves it until it has provided him with a positive or negative answer. And suddenly, the truth, emanating from him, explodes in such radiance that one cannot reasonably refuse him the title of "creator," or "discoverer." Such, in my opinion, was the case of William Harvey.

From Huang-Ti to Harvey

FELIX BOENHEIM*

IN the eighteenth century the Jesuit Father du Halde[1] reported on the Chinese claim to have known about the circulation of blood for at least two thousand years, if not since 2697 B.C.[2] The Chinese maintained that the philosophy of the macrocosm and the rotation of the stars which always return to the same place[3] applied also to men, because cosmos and man (microcosm) obey the same laws. Both are inseparable. Just as everything is subject to motion and mutation, so changes occur in the body and mind of man.[4] The Chinese point out that they have always been concerned with the close connection between respiration and the heart. According to their doctrine, there are twenty-three communications between the organs,[5] e.g. one vessel leads from the heart to the occiput.[6] The Taoists taught that the heart controls the body and is the seat of the vital spirit.[7] The blood which is neither arterial, capillary, nor venous is distributed by "Yie" and "Wang."[8] The heart is not the motor of the circulation but a receptacle. There is a mutual relationship between the blood and respiration and thereby with air.[9] Blood and breath are in continuous motion which begins in the lungs at 3 o'clock. As Chia-i ching puts it:[10] "The pneuma turns towards the lung; it flows inside and spreads to the outside. The Ching—quintessence—pursues its separate and unending course through the arteries as Ying—pneuma. This is defined as the order of heaven and earth . . ."

* Leipzig, Germany.

1 Du Halde, J. B. *Description géographique, historique, chronologique, politique et physique de l'empire de la Chine.* Paris, 1770, vol. 3, p. 385.

2 Murr, C. G. v. *J. Kunstgesch. allg. Litt.*, Nuremberg, 1777, pt. 4, p. 103.

3 Since the time of Aristotle, the circulation has played an important rôle in biology. Harvey advanced the view that the hen lays the egg, the chick emerges from the egg and develops into a hen.

4 Lieh-tse. Cited in: Forke, Alfred. *Geschichte der alten chinesischen Philosophie.* Hamburg, 1927, p. 296.

5 Scheube, Botho. Die Geschichte der Medizin bei den ostasiatischen Völkern. Pages 20-51 in: Puschmann, Th. *Handbuch der Geschichte der Medizin.* Jena, 1902-05, 3 vols. (Vol. 1, p. 25.)

6 Rousselle, Erwin. Ne Ging Tu, "Die Tafel des inneren Gewebes." Ein Taoistisches Meditationsbild mit Beschriftung. *Sinica*, 1933, *8*, 207-216. (P. 208, pl. 22.)

7 *Op. cit.* (note 4), p. 342.

8 Huard, P. and Wong, M. La notion de cercle et la science chinoise. *Arch. int. Hist. Sci.*, 1956, no. 35, 113.

9 *Ibid.*, p. 114.

10 I am indebted to Prof. Hübotter for calling my attention to this quotation.

The respiration is obviously more important than the heart because both animation and circulation of the blood are dependent upon it.[11] One man breathes 13,500 times within twenty-four hours, and the blood travels about 14 cm. during each respiration. The longest path which the blood travels in the body amounts to 437 mm. From this, it is calculated that the circulation is completed fifty times in twenty-four hours. Three streams influence the vital energy of man, namely, breath or respiration (Ki), semen or germ (Dsing), and spirit (Schen). It is, however, to be emphasized that although the first two are not identical with their bodily correlates, "breath" moves in the same rhythm as respiration.[12] According to Huang Ti Nei Ching, a physician of the T'ang H period,[13] "The blood current flows continuously and after fifty circulations the great union is established. Under healthy conditions the blood vessels are joined at both ends like a circle which has no beginning." It is evident that the Chinese did not know anything about the circulation of the blood in the Harveian meaning. Their doctrine is mystic, not biological.

The Chinese considered the blood to be the seat of diseases. If the patient had led a sinful life, the sin would accumulate in the blood. For a successful cure, as it was accomplished by the old Chinese physician Hoa t'ouo, the patient had to be provoked into such a state of rage that he vomited blood.[14, 15] Thus the blood was viewed as a defect, which was also a feature of Indian medicine in the middle of the first millenium A.D. This opinion explains both the bloodletting, which has been practised by all races since ancient times, and the medieval belief of changing the character of a man by blood transfusion. Statements concerning the knowledge of the circulation of the blood before Harvey are also found among other nations of the Far East which is only natural as a result of the close trade relations that also brought an exchange of cultural and scientific views between these countries.

The views of the Tibetans on the circulation of the blood can be gathered from a Lamaistic-Tibetan picture (*c.* 1050) repre-

11 Dabry, P. *La médecine chez les Chinois.* Paris, 1863, p. 2.

12 Rousselle, Erwin. Der Lebendige Taoismus im heutigen China. *Sinica,* 1933, *8,* 122-131. (P. 128.)

13 I wish to thank both Prof. Lee in Peking and Prof. Erkes for referring me to the text and for its translation.

14 Hübotter, Franz. Zwei berühmter chinesische Aerzte des Altertums—Chouen Yu-J und Hoa-T-ouo. *Mitteilungen der Deutschen Gesellschaft für Natur- und Völkerkunde Ostasiens,* Tokio, 1926, vol. 21, pt. A, p. 47.

15 Geiler mentions a similar story of a woman who had committed adultery. Venesection was successfully performed to remove the evil from her blood. (See Peters, H. *Der Arzt und die Heilkunst in der deutschen Vergangenheit.* Leipzig, 1900, p. 41.)

senting the wheel of life and death. A monster carries the earth, depicted as a circle which consists of seven unequal parts. In the centre are an eagle, a bull, and a snake. In a similar picture a pig, a dove, and a serpent are shown as symbols of the main vices. In a representation of hell I noticed a uterus-shaped structure, and on another picture Buddha forms the centre part.[16]

According to the Atharva-veda, 101 arteries lead to the heart which is the root of ten ducts. This concept is similar to that of the Chinese which holds the heart not as a pump but as a receptacle for the blood produced in the liver and in the spleen.[17] The Indian physicians of ancient times may have looked upon the heart as the centre of the circulatory system.[18] It is also of interest that their opinion coincided with the Chinese one—that the heart occupied a position superior to that of the brain because "the heart is the only place of consciousness."[19]

In ancient Egypt the action of the heart had always played a major rôle.[20] "It thinks everything that it wills whilst the tongue gives orders by its own will." The sensory organs inform the heart of exterior events.[21] According to Breasted, the oldest Egyptian physician of about 2800 B.C. (whose name is unknown) was very close to the discovery of the circulation of the blood. The heart acting as a pump distributed the blood, but Breasted himself adds elsewhere that the papyrus "does not indicate a recognition of the circulation of the blood."[22]

Galen's views are in many respects similar to those of the Chinese. His theory of the movement of the blood, with its concepts of vital spirits, dominated medical thinking.[23] He divided them into three kinds: the psychic spirit which has its seat in the brain, the vital spirit which mixes the blood in the heart, and the natural spirit which is distributed through the veins. This doctrine was accepted by later writers and was enlarged by Mondino who described in his *Anathomia* the natural members or parts associ-

16 In the Campo Santo in Pisa there is a picture three hundred years earlier resembling the Buddhistic representation. (Heinze, R. *Rede zur Feier des Reformationsfestes und des Überganges des Rektorats.* Leipzig, 1921.)

17 Kutumbiah, P. The concept of heart and blood vessels in ancient Hindu medicine. *Indian J. Hist. Med.*, 1956, *1*, 11-20. (P. 16.)

18 Müller, Reinhold F. G. Die Herz-Lehre der altindischen Aerzte. *Janus*, 1937, *41*, 261-93. (P. 264.)

19 *Ibid.*, p. 282.

20 Piankoff. *Le "Coeur."* Paris, 1930.

21 Grapow, Hermann. *Anatomie und Physiologie.* Berlin, 1954, p. 67.

22 *The Edwin Smith surgical papyrus.* J. H. Breasted, Ed. Chicago, 1930. 2 vols. (Vol. 1, pp. xvi and 3.)

23 Leibniz (see G. G. Leibnitii *Opera philosophica.* J. E. Erdmann, Ed., Berlin, 1840, pars altera, p. 475) characterizes "la correspondance de l'âme avec le corps" as a wonder.

ated with digestion, the spiritual members or thoracic contents, and the animal members or parts within the head.[24] Ibn an-Nafis (died 1288) discovered the pulmonary or lesser circulation but continued to assume that there was a communication between the right and left ventricle; his work corrected some of Galen's mistakes.[25] Although Mondino was the first anatomist in the Christian era who dissected bodies, he taught that the heart was situated in the middle of the body[26] and that it was the task of the lungs to cool the heart.[27] The blood always returned to the heart.[28] Leonardo da Vinci attached more importance to the mechanics of the circulation than to the spirit. According to him "the heart is a marvellous instrument invented by the supreme master;" it is situated half-way between the brain and the sex glands[29] and the veins originate from the heart. "The tears come from the heart and not from the brain."[30] One of his sketches shows a venous link between spleen and liver. This view corresponds to the Chinese idea of communications between the organs.[31] He rejects, however, the old teaching that air flows through the trachea into the heart.[32]

The knowledge of the movement of the blood has also been attributed to Leonardo, the most versatile genius of the Renaissance and the most remarkable mind in history. This assumption has been based on the following quotations: "Come il sangue chetorna indietro, quando il cuore si apre non è quel, che riserra le porte del cuore,"[33] and "The waters return with constant motion from the lowest depths of the sea to the utmost height of the mountains, not obeying the nature of heavier bodies; and in this they resemble the blood of animated beings which always moves from the sea of the heart and flows towards the top of the head."[34] These

[24] Esche, Sigrid. *Leonardo da Vinci: Das anatomische Werk.* Basel, 1954, p. 153 (Ars Docta, vol. 8).

[25] Meyerhof, Max. La découverte de la circulation pulmonaire par Ibn an-Nafis, médecin arabe du Caire (XIII[e] siècle). *Bull. Inst. Egypte,* 1934, *16,* 33-46. (P. 42.)

[26] Keele, K. D. *Leonardo da Vinci on movement of the heart and blood.* London, 1952, p. 57.

[27] Holl, M. Die Anatomie des Leonardo da Vinci. *Arch. Anat. Physiol., Anat. Abt.,* 1905, no. 1, 245.

[28] Holl, M. Untersuchung über den Inhalt der Abhandlung Roth: Die Anatomie des Leonardo da Vinci. *Ibid.,* 1910, 320.

[29] Leonardo da Vinci. *Quaderni d'anatomia.* C. L. Vangensten *et al.,* Eds. Christiania, 1911-16. 6 vols. (Vol. 5, p. 20.)

[30] Leonardo da Vinci. *The literary works.* J. P. Richter, Ed. London, 1883. 2 vols. (Vol. 2, p. 117.)

[31] Dr. Käte Finsterbusch and some other historians of art point out similarities between Chinese pictures and the background of Leonardo's paintings, e.g., Mona Lisa.

[32] *Op. cit.* (note 26), p. 50.

[33] *Op. cit.* (note 28), p. 320.

[34] *Ibid.,* p. 132, no. 849.

quotations do not justify the assumption that Leonardo was the founder of the theory of the circulation of the blood.

Leonardo also extends the theory of the blood movement to metabolism.[35] The body continually dies and is continually renewed by nourishment; therefore in order to prevent destruction, it has to be supplied with as much nourishment as it needs to support life. It is thus that life has its ebb and flow. Basing his views on Ptolemy's cosmography, the microcosm plays for Leonardo the rôle it had for the cultures mentioned earlier.[36] Without doubt Leonardo[37] knew the works of Galen and Avicenna.[38]

Klebs, in a critical review of Leonardo as an anatomist, rightly objects to his "vital principle" references which he would like to exhibit, along with "natural and animal spirts," in any museum of antiques. Leonardo tried to rid himself of the doctrine of the spirits by concentrating on the mechanics of the movement of the blood. The time, however, was not yet ripe for this idea; even Harvey still wrote: "neque sanguis sine spiritu sanguis est, sed aequivoca et cruor,"[39] as did Servetus before him, who had discovered the pulmonary circulation in Europe.[40] Harvey's starting point was Aristotle, who compared the weather and the rain to a circulatory movement in which the temperature of the earth, as influenced by the sun, plays a large part. He writes:

> And so, in all likelihood, does it come to pass in the body, through the motion of the blood; the various parts are nourished, cherished, quickened by the warmer, more perfect, vaporous, spirituous, and, as I may say, alimental blood; which on the contrary, in contact with these parts, becomes cooled, coagulated, and, so to speak, effete; whence it returns to its sovereign the heart, as if to its source, or to the inmost home of the body, there to recover its state of excellence or perfection. Here it resumes its due fluidity and receives an infusion of natural heat—powerful, fervid, a kind of treasury of life, and is impregnated with spirits, and it might be said with balsam; and thence it is again dispersed; and all this depends on the motion and action of the heart.
>
> The heart, consequently, is the beginning of life; the sun of the microcosm, even as the sun in his turn might well be designated the heart of the world; for it is the heart by whose virtue and pulse the blood is moved, perfected, made apt to nourish, and is preserved from corruption and coagu-

35 Leonardo da Vinci. *I manoscritti della Reale Biblioteca di Windsor. Dell'anatomia.* F. V. Sabashnikov and G. Piumati, Eds. Torino, 1901, pp. 170-2.

36 *Op. cit.* (note 26), p. 37.

37 *Op. cit.* (note 27), p. 183.

38 *Op. cit.* (note 29), vol. 3, p. 6, no. 5.

39 Harvey, William. Letter to Riolan. In: *The Works.* Robert Willis, Ed. London, 1847, pp. 109, 117, 118.

40 Boenheim, Felix. Die Entdeckung des Blutkreislaufs. *Zschr. ärzt. Fortbild.*, 1953, 47, no. 22, 807.

lation; it is the household divinity which, discharging its function, nourishes, cherishes, quickens the whole body, and is indeed the foundation of life, the source of all action.[41]

Although Harvey described only two half circles, although Malpighi described the capillary anastomosis between small arteries and veins, although Leeuwenhoek discovered the red blood corpuscles, although Boyle, Hooke, Lower, and Mayow recognised the proper function of the lungs, it is Harvey who deserves the distinction and honour of having discovered the circulation of the blood. Despite Morgagni's remarks,[42] "I do hope you do not belong to the close followers of Harvey" and the derogatory statements of other already forgotten medical authors of the eighteenth and nineteenth centuries, it was Harvey's logic that proved the existence of the circulatory system. He showed by simple calculation that the heart in one hour ejects a quantity of blood which is three times the average weight of man, and he concluded that the blood must flow in a closed circuit.

I have pointed to the similarities between the ancient Arabic and the more modern European views. But the question remains unanswered whether the different nations developed the concept of the movement of the blood independently, or whether they took it over from each other. Which nation can claim priority? For thousands of years Babylonian medicine believed firmly in a connection between earthly events and those observed in the sky. This led to an association of medicine with astrology. Astrology in turn was the connecting link between medicine and the natural sciences. It is known that the Near and Far East communicated with each other from time immemorial, and most certainly at the time of the Chaldaeans.[43] In Buddhistic literature there are several older legends which correspond to those of the Bible such as the story of the poor widow's mite or the feeding of the five thousand. According to both Dahlmann and Heck there existed in the first century A.D.[44] a lively international trade between Orient and Occident.

41 Harvey, *op. cit.* (note 39), p. 47.

42 Morgagni, G. B. *Von dem Sitz und den Ursachen der Krankheit.* Altenburg, 1771. 2.Abt., II.S. 965 XXV 18.

43 It is assumed that ships at the time of Homer had already sailed to India. According to Homer's *Odyssey (Odyssee.* J. J. C. Donner, Ed. Stuttgart, 1865, p. 51) there existed some dependence of Greek physicians upon Egyptian doctors. A fable states that Menelaus arrived in India by going round Africa (see Dapper, Olfert. *Die unbekante Neue Welt.* Amsterdam, 1673, p. 1). Darius sent seafarers to India. A medical paper before the time of Alexander the Great dealing with diagnosis and prognosis was also known in Babylon (see Filliozat, J. Ayurveda and foreign contacts. *Indian J. Hist. Med.*, 1956, *1*, 1-10).

44 Heinze, *op. cit.* (note 16), pp. 59, 60.

In 1202 Leonardo of Pisa compared the different sources of Arabic mathematics and our own. From investigations of two cuneiform texts, one from the early Babylonian, the other from the Hellenistic period, it has been concluded that "it would appear that European knowledge of it came through the Moslems from China."[45]

The Greek manuscripts had a peculiar history. They were translated into Arabic, reached Europe through the Arabic conquests where they were translated into Latin. It is also very likely that they reached the Far East, while the Indian texts went the other way round. Today the question whether the Indian doctrine of prana is older than the Greek idea of pneuma is unanswerable. But it cannot be denied that many Indian elements are in full agreement with the teachings of Plato.

Pneuma corresponds exactly to *prana*, chole secretion of the liver represents the fire and corresponds to the sanskrit *pitta*, and *phlegma* to the *slesman*. The correspondence between the secretion of the liver or *pitta* and the fire is a very special theory, and certainly a very ancient one in India, as already fire, agni, is referred to as the pitta of waters in the Yajurveda. Furthermore, many details on pathology are similar in Plato's doctrine and in the Ayurvedic system. Thus, it would be rather astonishing if Plato or the first Greek exponent of his theory conceived, by himself, the doctrine currently professed by the Ayurvedic physicians. It is more plausible that he got a notion of it, as it was a common doctrine among Indian physicians and thus well known in the Indian territory of the Persian Empire, which extended up to some Greek countries.[46]

It is well known that relations existed between India and China. Bodisharma travelled from India to China in 520 A.D. The influence upon Tibet is also known. In the seventh and eighth centuries Arabic merchants traded with India and Egypt.[47] To illustrate the influence of the Arabs, who were the standard bearers of the entire Asiatic culture in the Middle Ages, it is sufficient to mention the name of Constantinus Africanus who during his travels might have reached India, but later, after having had to flee from Egypt, spread the knowledge of the Arabs in Salerno.

Even though anatomical knowledge had made great progress, in 1501 Magnus Hundt still demonstrated in his anatomical woodcuts the topography of the various organs as imagined in the early Middle Ages.

[45] Mason, S. F. *A history of the sciences.* London, 1953, p. 87.
[46] Filliozat, *op. cit.* (note 43), p. 7.
[47] Mason, *op. cit.* (note 45), p. 71.

Erkes[48] compared the mysticism of the Taoists with that of Angelus Silesius and emphasized the great similarity to primitive Shamanism. It is possible that Harvey, who had studied in Padua, was familiar with the Arabic views. But like his predecessors, he could not have based his physiological doctrine of the circulation on the Asiatic models, because the ancient cultures did not have the idea of the movement of blood as he understood it. Harvey's work crowns with its logic the ideas of all scholars that went before him.

[48] Erkes, E. Mystik und Schamanismus. *Artibus Asiae*, 1945, *8* (nos. 2-4), 197-215.

Early Accounts of the Valves in the Veins *

JOSHUA O. LEIBOWITZ**

After my death I wish no other herald,
No other speaker of my living actions,
To keep mine honour from corruption,
But such an honest chronicler as Griffith.
Shakespeare, *King Henry VIII*, IV, ii, 69.

IT may not appear presumptuous to prefix a Shakespearean motto to the present study on pre-Harveian and even pre-Fabrician work on the valves in the veins. The great poet would willingly have shed the light of his verses on the "living actions" of the great physiologist, his younger contemporary William Harvey. He would likewise have let the beauty and force of his poetry shine on those minor explorers in the narrower field under discussion whose work and "living actions" deserve recognition of an "honest chronicler."

Historical Limitations in Biological Discoveries

Viewed historically, a discovery in biology, if valid and proven, holds its ground even when the discoverer failed in his explanation or when the possibility of correlation with other closely associated findings was yet to be made. Even Harvey's epoch-making discovery of the circulation of the blood is not exempt from some historical limitations. They may have been emphasized, perhaps unduly, by F. H. Garrison in the third edition of his even now unparallelled *History of Medicine* (pp. 243-4): Harvey "adhered to the old doctrine that the function of respiration was to cool the hot blood"; "in his attempts to explain the function of the blood and the lungs Harvey fell into the usual medieval mysticism." A more judicious evaluation is that by Fulton who gave a concise description of "the revolution in medicine which Harvey introduced by reviving the experimental method."[1] Fulton outlined "the important discoveries lying ahead: the capillaries, the composition of the blood, its pressure in the arteries, the action of the heart as a pump, and the volume of output of the beat."

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1 Fulton, J. F. *Physiology*. Vol. V in *Clio medica* series on the history of medicine. New York, 1931, pp. 32-3.

The question of limitations has been brought forth in order to attain an appropriate evaluation of the early descriptions of the valves in the veins. None of these descriptions approached the physiological significance of the valves: Harvey's correct interpretation lay ahead.

THE VALVES IN THE VEINS A STARTING POINT FOR HARVEY

We follow the inquisitive mind of Robert Boyle[2] when we ask with him what were the things that induced Harvey to think of a circulation of the blood. The passage in Boyle seemed so important to Fulton that he chose to prefix it to his history of physiology mentioned above. Harvey's answer was this: that "the Valves in the Veins gave free passage to the Blood Towards the Heart, but oppos'd the passage of the Venal Blood the Contrary Way."

Harvey had already in 1616 referred to the valves in the veins in a short passage in the manuscript of his Lumleian Lectures. The simple experiment of using a tourniquet to visualize the valves was considered by him the first demonstration of the "perpetuum sanguinis motum in circula ab arteriis ad venas." Since the existence of valves in the veins had a firm place in the thought-process of Harvey, the early history of the work on them may be of some interest. As K. J. Franklin recently pointed out: "No advances in physiology have been really sudden."[3] This stimulating paper refers in a note (pp. 495-6) to the item under discussion.

CANANO

The first account of the valves in the veins published in some detail[4] was given in 1551 by Amatus Lusitanus (1511-68) who was then lecturing at the University of Ferrara.[5] He demonstrated valves in the vena azygos in the course of dissections. One of the dissections was attended by Giambattista Canano (1515-78) whom he especially mentioned at the end of the account. Since Canano himself did not publish any statement about the valves in veins

2 Boyle, Robert. *A disquisition about the final causes of natural things.* London, 1688, pp. 157-8.

3 Franklin, K. J. History and the physiologist. In: *Science, medicine and history, . . .* in honour of Charles Singer, E. Ashworth Underwood, Ed., London, 1953, vol. 2, pp. 494-500. See also: Osler, William, The growth of truth. Harveian Oration, London, 1906, pp. 6-11 ("acquisition, latent possession, conscious possession").

4 A vague mention of valves in the portal vein is found in: Estienne, Charles, *De dissectione.* Paris, 1545, lib. II c.9, p. 182, line 44. Estienne feels that "the apophyses membranarum are placed in order to protect the liver from pain should the blood some time reflux by some inconvenience." [!] (French edition 1546 used.)

5 Amatus Lusitanus. *Curationum medicinalium centuria prima.* Florence, 1551.

and since the credit given to him rests upon his personal communication to Vesalius, it appears appropriate to reproduce in translation Vesalius' report. A few lines therefrom have been quoted several times,[6, 7, 8] but not the whole text. Vesalius referred to this matter in his book *Anatomicarum Gabrielis Falloppi observationum examen* which was written in 1561 and published posthumously in 1564. Because I felt that this passage had been written in rather difficult Latin I asked Dr. G. L. Hendrickson, Professor Emeritus of Latin and Greek Literature at Yale University, to translate it for me. I am happy to thank this distinguished scholar for his friendly help.

[In vena pari carente.][9] Whether in this matter Canano was jesting and, as you [Falloppio] are convinced, was somewhat unfairly trying to get a laugh at my expense and Amatus' and several others, I cannot say. At all events at Ratisbon, when we were both called in to visit Francis Este in his illness, he reported to me that at the beginning of the *vena conjuge carentis*, and also at the orifice of the veins going to the kidneys, and found at the *elatiorem sedem* of the *os sacrum*, he had observed valves [*membranes*][10] of the same kind as are found at the beginnings [principiis] of the arterial vein and of the great artery, and affirmed that these valves prevented the blood from flowing back [sanguinis refluxui obstare]. From this statement [unde] I was impelled soon after to consider by dissection whether the matter was in fact true. I learned that Amatus was also of the opinion of Canano, and I read that he depended on Canano for his own judgment, at the end of his [Amatus'] chapter, in which he contemplates [prospexit] the nature[11] of the veins in their distribution to this swelling [robori]. I continued on [persequor], and I added to his account tolerably clearly what I thought should be determined concerning these valves [membranis]. For the valves themselves I did not find, but in the very orifice of the body of the vein I discerned a certain thickening [crasities] worthy of note, and a swelling [extuberationem] to be called *robur*. This then, rather than valves [membranarum vice] was visible to all who saw it, as I have described it [scripsi].

This report by Vesalius yields some material for comment. The date of his meeting Canano at Ratisbon is not mentioned, but it seems safe to assume that it took place in midsummer 1546.[12] Vesalius' . . . *Examen*, the last book from his pen, had been completed in December 1561; the author was in a hurry to have it

[6] *Joannes Baptista Canano . . . Musculorum humani corporis picturata dissectio.* Facsimile edition, annotated by Harvey Cushing and Edward C. Streeter. Florence, 1925, p. 29.

[7] Fulton, *op. cit.*, p. 21.

[8] Friedenwald, Harry. *The Jews and medicine.* Baltimore, 1944, vol. 1, p. 272.

[9] P. 82, bottom of page (Num Cananus).

[10] Note from S. Alberti, *De valvulis*, 1585, p. 1: "Formerly called *membranae* now (1585) valvae atque ostiola."

[11] Text *natura* here translated as if natur*am*.

[12] Roth, M. *Andreas Vesalius Bruxellensis.* Berlin, 1892, pp. 207-8.

ready to be sent by the Venetian ambassador at Madrid, Paolo Tiepolo, to Falloppio. There are therefore some inadequacies in style and grammar, as Professor Hendrickson noted in a personal communication.

It is clear that Vesalius in his criticism of the valves in the azygos vein refers in the same way both to Canano and Amatus. He mentions Canano twice and Amatus four times (pp. 82-4). He admits to having seen swellings in the veins but no valves as in Canano's verbal or in Amatus' printed record.

One item may be stressed in regard to Vesalius' biography. It appears that he did not stop anatomizing after his retirement from Padua. True, the last page (171) of the *Examen* indicates the years of inactivity of the great anatomist at Madrid: ". . . No opportunity to undertake a dissection can occur here (where I cannot conveniently obtain even a skull)." However, this rather sad portrayal is not valid with regard to the years immediately after his retirement from the Paduan chair. Following Canano's verbal communication, Vesalius "was impelled soon after to consider, by dissection, whether the matter was in fact true." And after having read Amatus' report, which appeared in 1551, he "continued on and added what he thought should be determined [*sc.* anatomically] concerning these valves." In fact, Vesalius did not move with Philip II to Madrid before 1559.[13] Thus, from the report in the *Examen* (p. 83) we may also learn something about the post-Paduan dissecting activities of Vesalius.

AMATUS LUSITANUS

As previously mentioned, the valves in the azygos vein had been described by Amatus at some length in the "Scholia" appended to the 52d Curatio of his first Centuria.[14] Finished in 1549 at Ancona, this Centuria was published at Florence two years later. The case history was that of a patient with pneumonia and is not remarkable save for a profuse epistaxis as a toxic manifestation and an epidemic occurrence of similar cases.[15] The patient had an uneventful crisis and was cured ("in copiosissimum sudorem devenit et sanus factus est"). Of course the patient was bled according to

[13] *Ibid.*, p. 373.

[14] *Op. cit.* (note 5).

[15] The disease is called here pleurisy ("pleuritide correptus"). Other older physicians used to call it "dolor lateralis" as Vesalius' title of his Bloodletting Epistle shows, though pneumonia was mentioned already by Arataeus, Galen, and Aëtius (Garrison) usually called "peri-pneumonia." It seems that the first mention of "pneumonia" in an English medical book is that by W. Cullen as late as 1783 (*Oxford English Dictionary*).

ANDREAE
VESALII,
ANATOMICARVM
GABRIELIS FALLOPPII
OBSERVATIONVM
EXAMEN.

CVM PRIVILEGIO.

VENETIIS,
Apud Franciſcum de Franciſcis, Senenſem.
M D LXIIII.

FIG. 1. Title-page of Vesalius' *Examen* on Fallopius, 1564, which carries the autograph of Caspar Bauhin, dated Padua, 1577. This copy is now in the Yale Historical Library.

AMATI

LVSITANI

SVMMI DOCTORIS

MEDICI CVRATIONVM

MEDICINALIVM CENTVRIÆ SEPTEM

AB OMNI SORDIVM SVSPICIONE EXPVRGATÆ,

QVIBVS PRÆLVCET OMNIVM CVRATIONVM PER locos affectos in ſua capita digeſtarum, una cum appendicibus eorum, quæ auctor, vel inter curandum, vel in Scholijs accuratè diſſerit, proprijs capitibus ſubiunctis

ILLVSTRIS ET CERTVS INDEX

In Bibliotheca Iohannis Franciſci Roſſelli Catalani Barcinonenſis Reperitur.

Anno 1628.

CVM LICENCIA.

BARCINONÆ. Sumptibus SEBASTIANI & IACOBI Mathevats.

FIG. 2. This title-page is from one of the last editions of Amatus to be published (first edition 1551) and is the only one printed in Spain. (The same printers, Sebastian and Jacob Mathevats, issued at Barcelona, 1627, Rosselli's commentaries on Galen's *De differentiis et causis morborum et symptomatum*.") Note the line: ab omni sordium suspicione expurgatae [freed from all sordid suspicion], a fact referred to also in the *Approbatio* through the "Holy Office of the Inquisition." Note also the printer's device: a turtle protected by its shell against a swarm of bees, with the motto "Non penetrant." It may be that the unusual device is based on Ps. 118, 12, "They compassed me about like bees," thus symbolizing security against damage. (Animal signs have been reproduced in H. W. Davis' *Devices of early printers*, London, 1935.) The copy used is in the Yale Historical Library; the Wellcome Historical Medical Library also possesses this edition.

ADVMBRATIO OSTIOLORVM, VT SESE ILLA OBTVLERE IN CADAVERE ANNO 79 menſe Decembri diſſectioni noſtræ ſubjecto.

PRæſens figura crura petentis venæ inflatæ & ſupra ſubterq; abtruncatæ portionem commonſtrat, quemadmodum foris ea apparet. Ac A quidem ſuperiorem eius regionem propè poplitem. B. vero inferiorem & humiliorem: C. ramum indicat è dextro latere forâs prorumpentem in muſculos, ut nutrimento eius educentur.

D & E. valvulæ ramo illi ſubjectæ dimidiam duntaxat faciem notant forâs quaſi in loculum protuberantem, in quo morari aliquantiſper ſanguis poſſit.

Hæc

FIG. 3. Page 14 (unnumbered) from Salomon Albertus' *Tres orationes*, Nuremberg, 1585; this is one of the last pages of the small tractate, *De valvulis membraneis quorundam vasorum*, appended to the *Orationes*. The Latin title "Adumbratio" means a sketch in shadow, a perspective sketch or draft. The figure has not been enlarged. The copy used is in the Yale Historical Library.

FIG. 4. Alberti medal. From J. C. W. Moehsen's *Beschreibung einer Berlinischen Medaillen-Sammlung*. Title above the engraving: "Medaille des Churfuerstlichen Saechsischen Leib-Arztes, Salomon Alberts." Five pages of text, including a record of the description of valves in veins (p. 27). The medal was cast in 1596, probably at Dresden. Its peculiar feature is the combination with the figure of Alberti's wife, Ursula, née Beurin. This was not usual, but was sometimes done, as the figure on page 73 of the same volume shows. No other portrait of Alberti has been traced by the present writer. Copy used is in the Yale Historical Library. Slightly enlarged.

the custom; but he was young (thirty years old) and was described as "vir robustus," so the phlebotomy did not involve much damage. But there arose the vexed question, whether the venesection should be made on the side of the chest pain (Amatus' view) or always on the right arm (Vesalius' view in his Epistle on Bloodletting).[16] This question of the side for phlebotomy, now obsolete and irrelevant, was the topic of endless discussions in the sixteenth century and centered on the activities and views of Pierre Brissot (1478-1522).[17] Thus, in the Scholia appended to the case history ("at the end of his Chapter" in Vesalius' words), Amatus enters into a discussion which culminates in the first description of the valves in the azygos, to which vein Vesalius had referred in his early Epistle:

> . . . For the azygos vein does not send back more blood than it receives from the cava; on the contrary such is its fashion at its ending that it possesses valves (ostiola) which open for the (influxing) blood, and close afterwards, thus not allowing the received blood to flow back. It comes out that the azygos vein proceeds much as the urinary bladder, or the orifices of the heart vessels. What is proper to the azygos vein, namely, that it does not in another way let flow back the blood which it received, this we have ascertained by the dissection of bodies. For when you cut the vena cava at its upper part and by inserting a tube you blow downwards, both the inferior part of the cava and the azygos will be inflated and become tumid; but if you cut the azygos at its lowest part and by introducing a tube or a cannula you press against the upper part, certainly the vena cava will not inflate nor become tumid, because the air contained in the azygos cannot go out, on account of valves, called also opercula, which it possesses at its orifice; whence it is clear that if air cannot pass out of the azygos into the vena cava it is all the more certain that blood, much thicker than air, cannot pass through. That indeed these things which I have said will happen to the azygos vein is certain; nor can there be any doubts in this since we proved it a thousand times. For in the year 1547 we caused twelve corpses of men and animals to be dissected, and in all of them it happened just that way in the presence of a great company of learned men looking on: as at the same time [place? matter?—"ibidem"] likewise observed Giovanni Battista Canano, that admirable anatomist. . . .
>
> . . . Please accept therefore, you candid men, our account which is not indeed conjectural or sophisticated, but true and based on the very dissection of the human body.

It is clear that Amatus did not understand the physiological significance of the valves in the veins any more than did all the other authors before Harvey, including Fabricius. However, his is the first published description of these anatomical structures.

[16] Vesalius, Andreas. *Epistola docens venam axillarem dextri cubiti in dolore laterali secandam.* Basel, 1539.

[17] See Roth (note 12), p. 32; also Garrison's *History of medicine*, 3d ed., pp. 226-7.

Estienne's vague mentioning may be passed over here. Amatus clearly stated that the valves retard the back flow of the blood. It was unfortunate to discuss the valves in the azygos vein rather than the clearly demonstrable valves in the veins of the limbs. In fact, modern anatomists mention the azygos among the veins possessing few and often imperfect valves. This explains why neither Vesalius nor Falloppio could confirm the observations of Amatus who chose the azygos as the object of his dissections merely to clarify those questions about bloodletting which had been raised in Vesalius' early Epistle.

ALBERTI

The third author to be named is Salomon Albert, called by his bibliographers Alberti (1540-1600). It is not easy to explain how the name became changed to Alberti, probably on the authority of Haller.[18] Van der Linden[19] spells his name Albert (Latin Albertus), as do Jöcher[20] and Portal.[21] Eloy[22] uses Albert, but in his later edition[23] he adds "ou Alberti." Alberti himself always used the Latin form "Albertus" which corresponds to Albert. And thus his name appears on the medal coined for him in his lifetime. However, since Haller's *Bibliothecae* possessed the greatest authority, his spelling was generally adoped, just as his error in asserting that Alberti was a pupil of Fabricius ab Aquapendente crept also into Portal's History. On the other hand, Moehsen,[24] having first-hand information as a fellow countryman, persisted in the original spelling. He also contributed more material to the life history of that remarkable anatomist and physician. This is worth mentioning since the title of the book does not indicate the wealth of biographical details. The entries on Alberti's name in the dictionaries of medical biography are uniformly poor, while the bibliographical data are adequate.

18 Haller, A. *Bibliotheca anatomica.* Leyden, 1774-7. 2 vols. (Vol. I, p. 251.)

19 Linden, J. A. van der. *De scriptis medicis libri duo.* 3. ed. Amsterdam, 1662.

20 Jöcher, C. G. *Allgemeines Gelehrten-Lexikon.* Leipzig, 1750-51. 4 vols. (Vol. I, p. 209.)

21 Portal, Antoine. *Histoire de l'anatomie et de la chirugie.* Paris, 1770-3. 6 vols. (Vol. II, p. 88.)

22 Eloy, N. F. J. *Dictionnaire historique de la médecine.* Frankfurt, 1756. 2 vols. (Vol. I, p. 34.)

23 Mons, 1778. 4 vols. (Vol. I, p. 62.)

24 Moeshen, J. K. W. *Beschreibung einer Berlinischen Medaillen-Sammlung, die verzuegleich aus Gedaechnismuenzen beruehmter Aerzte bestehet.* Berlin and Leipzig, 1773-81. 2 vols. (Vol. II, pp. 25-30.)

Alberti's description of the valves in the veins is contained in the last fifteen unnumbered pages of *Tres orationes*.[25] The interesting account is too long for reproduction. A few sentences may be translated:

> . . . Let me, however, return to the membranes in the veins. Amatus, in the Scholia to the 52nd curatio of the first centuria, says that he saw in several bodies ostiola or opercula at the beginning of the azygos vein so that blood that had once been absorbed might not reflux or regurgitate . . . Amatus relies on the authority of Giovanni Battista Canano to whom Vesalius said he communicated the same. . . . [Then Falloppio and Eustachio are mentioned who opposed the view.] . . . Now this is what we teach as the most important thing to be said about the valves. . . . It has been related that beginning with the year 1579 Hieronymus Fabricius pointed out valves in brachial and crural veins. . . . This is what I perceived through hearing or through letters containing Fabricius' observation which my best friend, the learned Georgius Palm, physician at that celebrated [Paduan] University, sent me to Nuremberg. In the same year, 1579, I myself, teaching at our anatomical theatre, demonstrated the valves in the veins. Shortly after, Fabricius demonstrated them at Padua. . . . [Then follow the technique of preparation and a detailed account.]

Alberti's account of the valves in the veins, mentioned in Haller, has recently been commented on aptly by K. J. Franklin[26] who reproduced, markedly enlarged, the two illustrations inserted in the original account. The figures are much inferior in quality to the excellent copper plates used by Fabricius in the original edition of 1603, but they "are quite remarkably typical of such structures."

"The End of the Matter"

Three scholars of the sixteenth century spoke their thoughts, one of them through the hero of anatomy as an intermediary. These pre-Fabrician men of the healing art have much in common —their enthusiasm for anatomical dissection and their helplessness in physiological explanation. Canano is no exception: [in Vesalius' paraphrase] "The valves prevented the blood from flowing back" (sanguinis refluxui obstare). These are the same words used by Amatus and Alberti. But the direction of the blood stream is wrongly conceived even as it was in Fabricius' attempt at explanation. E. C. Streeter in his biographical notice tries to exempt

[25] Alberti, Salomon. *Tres orationes*. Nuremberg, in officina typographica Catharinae Gerlachiae, 1585. After the death of her husband, Dietrich, the enterprising Catharine headed the printing press from 1575 until 1592.

[26] Fabricius, H., *ab Aquapendente. De venarum ostiolis, 1603*. K. J. Franklin, Ed. Springfield, Ill., 1933, pp. 22-4.

Canano from the pre-Harveian error, but no documentary evidence is available. Dr. Streeter's style is rich and quite extraordinary. This has been pointed out by Dr. Fulton[27] in his biographical appreciation. Phrases about Amatus Lusitanus such as "But a fly was in the ointment" (p. 33),[28] "Amatus could cry 'mea culpa' " (p. 35), "Amatus published a screed" (p. 33), "he involved the name of that silent man in his own egregious blunder and confusion" (p. 34) do not sound very friendly. Actually, they reflect only the emotional style of the Renaissance which Streeter knew so well and loved so intently. K. J. Franklin[29] is right that Amatus "unfortunately has an erroneous idea as to the direction of the flow of the blood." We do not know that Canano's idea was different. It may appear petty to comment on the Latin words of Amatus in which Canano's name has been involved. When I asked Professor Hendrickson to translate "ut ibidem quoque adnotabat J. B. Cananus," he immediately rendered it "as at the same time [place?] likewise observed J. B. C." However, "ibidem" means also "in the same matter." And so Vesalius and Alberti seemed to understand the word *"ibidem"* when they referred to Amatus' published account. On the translation of a simple Latin word depends the notion of whether Canano was merely an observer or a previous discoverer of the valves in the veins.

I cannot resist concluding this study with a quotation from Osler's *The Growth of Truth* (London, 1906, p. 17) already quoted by Friedenwald (pp. 354-5). Speaking of the great scholar Fabricius, Osler deals with blindness to truth: ". . . his eyes were sealed and to him, as to his greater predecessors in the chair, clear vision was denied. The dead hand of the great Pergamite lay heavy on all thought . . . Not without a feeling of pity do we read of the hopeless struggle . . . to escape from slavish submission to authority." And I close with Franklin's words: "It was left to William Harvey to grasp the true significance of the valves, and to make the greatest single contribution ever made to physiological knowledge."

27 Fulton, J. F. Edward Clark Streeter, 1874-1947. A biographical appreciation. [New Haven] 1948. (Publication No. 20), The Historical Library, Yale University School of Medicine.)

28 *Op. cit.* (note 6).

29 Franklin, K. J. Valves in veins: an historical survey. *Proc. roy. Soc. Med.*, 1927, *21*, (Sect. Hist. Med.), 1-33, (p. 4.)

William Harvey, Disciple of Girolamo Fabrizi d'Acquapendente and the Paduan School

ADALBERTO PAZZINI*

HE errs who thinks that discoveries, whatever they be, come forth complete and fully fashioned from the mind of man like Minerva who sprang fully armed from the brow of Jove. Such portentous birth, albeit profoundly significant symbolically, is possible only among the gods, and we humans must be content with what our human nature grants us.

Too often historians quarrel among themselves in wanting to establish priorities which really do not deserve to be singled out, for in any disputed discovery so many elements of such diverse origin, transmitted through such unthought-of means, are merged, that it is impossible to establish where the achievements of an individual discoverer begin and end. When such co-operation—often unwitting—is met in one person, he officially receives the praise and glory of discovery, but it is the historian's duty to find in this egocentricity the various parts of which it has been composed. Discovery is born of great personal labor—labor of an entire era, sometimes of generations that have been preparing for it, and particularly in an atmosphere that favors its birth, so that it is not exceptional to see some discoveries die still-born in one age only to be re-created in another of different social and cultural atmosphere. The history of the discovery of the circulation is an example of this. Questions of priority have been raised, and continue to be raised, in which too often there have been elements that should not enter into such judgments.

The function of the blood was too complex and too controversial for the mentality of the ancients. Too many arguments having nothing to do with physiology came to be involved in it, thus touching upon, if not actually developing into, questions of philosophy and dangerous theological arguments. The movement of the blood was not simply an organic function, for the blood was the vehicle of the 'vital spirit' which was formed in the heart, reputedly the seat of the 'sensitive soul.' The destruction of this mighty concept which had endured Gibraltar-like throughout the

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centuries could not be wrought by one man, and anyone who defends this proposition ignores historical evidence.

Opposed to these formidable spiritual obstacles were other hindrances which, although on more accessibly human and purely scientific and biological levels, could still not be overcome by one man alone but, as I have already indicated, only by the collaboration of many elements. In addition, the right atmosphere was needed—an atmosphere able to meet the demands of a theory with power to give life to the discovery and to bring about its acceptance on a truly scientific basis. Such an atmosphere is not the creation of a single person, but is based on a gradual change that moulds a whole society into another *forma mentis.*

All this and still more applies to the discovery of the circulation, a function involving so many arguments, each of which had to be revised and, if necessary, destroyed. This must be borne in mind by anyone wishing to arrive at a concept of the history of the circulation which adequately approaches the truth without the absurd pretense of being the absolute truth.

He who reads Harvey's little book on the circulation (little in bulk but vast in scope) unquestionably realizes it to be a work truly worthy to impress itself on its own time and on posterity.

Disregarding, for now at least, the question of genuine originality of the ideas in the *De motu cordis,* and the possibility that the author set forth concepts that were not entirely his own, there remains the undeniably admirable organization of the work and, above all, the method used to prove his arguments, an experimental method that does not depend on the artificiality of the syllogistic and sophisticated reasoning so prevalent in the preceding generation.

He considered the entire circulatory system as an hydraulic-mechanical function; he performed autopsies on various kinds of animals, establishing a scale of biological comparisons of the less evolved and more perfectly organized animals—man included; he used a convincing mathematical method to demonstrate that, given the quantities (expressed in ounces, drams, and scruples) that are pushed into the circulatory system at each systole, the blood must circulate; and all this unquestionably indicates a mental stature on the part of the author that is indeed worthy of admiration and that without doubt far outdistances the earlier work of others. Harvey's book was published, as everybody knows, in 1628 when the experimental method was first in flower, a time characterized by a

genuine Galilean outlook which later, however, was dissipated in an experimental baroque.

To the discoveries and ideas of Vesalius, Falloppio, Colombo, Cesalpino, and Eustachio which had opened a new field for research in purely morphological anatomy, there was now added another element, that of dynamism and mechanics which Leonardo da Vinci had already introduced into his studies of the "fabbrica del corpo umano" but which had not developed further because his work was done in solitude and not made known for many years. But from the Galilean doctrine, "Nature is written in mathematical symbols," came a new and fertile impetus and from it either directly or indirectly scientific research was evolved.

Certainly, however, because of proximity, he who was closest to Galilei was to feel the benefit more directly. Galilei began teaching at Padua in September, 1592, and there his concepts grew and were diffused, leaving their mark on future scientific endeavor. If an understandable reserve deters us from making categorical assertions, we still cannot help noting that it was truly in Padua, through one of his teachers of anatomy, that this science gave the first sign of a new mechanical-dynamic concept. The teacher was Girolamo Fabrizi d'Acquapendente who in 1614 published a tract on human mechanics in which he applied physical laws to the various movements of animal organisms. There followed after four years another volume which even in title (*De motu . . . animalium*) preceded the more complex work of Borelli on the same subject. Fabrizi's *De venarum ostiolis* is another example of the application of hydraulic mechanics to the circulation. He was also the originator of comparative anatomy, for in his works he examined the organs of man and of various animals in order to emphasize the differences and similarities, his comparisons sometimes leading to paradoxes such as his *De brutorum loquela,* so entitled not because animals talk but because in it he compares the organs of phonation of the various species. And finally, Fabrizi, even though he was not actually the founder, was a dedicated student of embryology, and his *De formatione ovi et pulli* (1621), which appeared twenty-one years after *De formato foetu,* contains many of our modern concepts. A disciple of Gabriele Falloppio, he was one of the glorious line of Paduan teachers that began with Alessandro Benedetti and reached its height with Vesalius, Colombo, and Falloppio.

William Harvey graduated from Padua in 1602, having arrived there from Caius College, Cambridge, in 1598. Four years' resi-

dence in Padua and in the intellectual and cultural atmosphere of these surroundings could not but leave their mark on the twenty-year-old youth in his formative phase. Charles Laubry, who translated and commented on the *Exercitatio* (1950), correctly observed: "Harvey [in Padua] s'y imprégne de l'enseignement de Fabricius. . . . En même temps il y coudoie des étudiants épris comme lui de savoir et de recherches; il y trouve l'éternel auguillon des idées que représente la jeunesse, aussi spontanée dans ses admirations que dans ses critiques, toujours à l'affût d'une nouveauté et toujours empressée de la propager." Laubry is right; Harvey was formed at Padua, and his work is the direct result of that moulding. Harvey himself acknowledges this and frequently pays tribute to the Italians, especially to Fabrizi who was his teacher, to Colombo who first conceived of the theory of the lesser circulation, to Fracastoro, and to others.

As Laubry states, Harvey brought back from Padua many notes and much precious documentation, and in the course of the next twenty-six years he had time to meditate on this material which he eventually disclosed during that period when he was continuing his observations and lecturing on anatomy at the College of Physicians. As he himself states in regard to the *lesser* circulation, he first came upon the concept of the circulation at Padua, and slowly but surely this theory matured and became his own; but the mark of the Paduan school was permanently impressed upon him, his best work being the direct result of his studies at Padua under Girolamo Fabrizi d'Acquapendente, and this is evident in every detail. In the fundamental elements of Harvey's method which make his work a true masterpiece, it is indeed not difficult to recognize the salient points of his master's scientific conceptions. Like Fabrizi, who was reputedly the founder of comparative anatomy, Harvey made comparative observations of the various species of animals and used these comparisons better to demonstrate his assertions. As Fabrizi was among the first to excel in the field of embryology, which was then emerging from research on the fetus and the incubated egg, so Harvey had recourse to the same embryological observations on various animals, and especially on the fetus and the chick. Like Fabrizi, who was the first, perhaps, to introduce mechanical laws into the study of organic functions, he treated the phenomenon of circulation in terms of mechanical-organic-hydraulic fact and placed the entire weight of his argument on a

purely mathematical point of multiplication of the ounces of blood that are pushed into the circulatory system at each pulse.

Who cannot see in these similarities, for which the evidence is so clear, a definite connection between disciple and pupil? Who cannot see in Harvey's work the result of that vast Galilean doctrine which produced men such as Borelli, Malpighi, Santorio, Bellini, and links their writings with those of the Englishman? It is thus that Harvey's work is associated with that of our great men and resembles it because they shared the teachers and intellectual environment of the same school. I shall not cavil about whether Harvey knew of Cesalpino's writings and his ideas on the circulation; one might infer that he did from the fact that on this point he maintained an almost ostentatious silence, something unusual for those times.

What I wish to express here, is true and unconditional admiration for Harvey's work which was a direct result of the Paduan school where it seems that Fabrizi, who was himself not receptive to new ideas about the circulation, had indicated to his great disciple a scheme for his work and a method for carrying it out so that he could interpret the phenomena he observed according to his own concepts of embryology, comparative anatomy, dissection, mechanics, and mathematics.

Ben venga, then, the Italians who, according to Laubry, were moved by blind nationalistic enthusiasm; *ben venga* William Harvey's great work, a direct result of teaching which was not confined to a single nationality, but extended to the world—the teaching of the Paduan school whose rich yield is displayed in the magnificent writings of one of its most illustrious pupils, William Harvey.

The Reception of Harvey's Doctrine in Denmark*

EDV. GOTFREDSEN**

IT is a matter of general knowledge that William Harvey's dissertation on the circulation of the blood, when published in 1628, created a great sensation but did not gain unwavering support. Above all, the older generation had some difficulty in understanding it. Among the juniors some swore enthusiastically by the new theory, while others hardly knew what to believe. Both types are characteristically represented in a letter written at Leyden on 13 July 1631, by a Danish student, Jacob Svabe, to his former teacher in Copenhagen, Oluff Worm. The letter also bears testimony to the incredulity of the older generation. Newly acquainted with the Harveian theory, Svabe writes:

> This doctrine so greatly impressed my mind that, for a full week, I was quite heartsick owing to these profound thoughts. Hardly being able to calm myself by my own efforts, I revealed the matter to my friend Conring, who is an industrious student of medicine. Having been shown Harvey's dissertation he explained the circulation of the blood so admirably and plainly, that he himself almost seemed to be of the same heretical opinion. He soon perceived, however, that, being very desirous of a new thing, the mind inclines to be too much titilated and allured, and now he said: "Certainly this explanation in itself is elegant, and, at first glance, highly probable. If Harvey had only been able to prove it by means of autopsy and anatomical demonstration, he would have solved the whole problem." After that, I took myself to the famous men, van Heurne and van Valkenburg. They were willing to accept the opinion of Conringius, with all their hearts, as the saying is, if they had not to add that in changing old theories and approving of new ones we must rather be timid and hesitating than audacious and temerarious."[1]

These middle-aged professors had dismissed the young man with platitudes, but their doubts were clear enough. In answering this letter, the forty-three-year-old Worm shows his agreement with the scruples of his colleagues. He writes on 25 September 1631:

* Abridged from *Acta medica scandinavica*, 1952, *142*, suppl. 266, pp. 75-86.

** Copenhagen, Denmark.

[1] Worm, Oluff. *Wormii Olai et ad eum doctorum virorum epistolae* (= *Epistolae Wormii*). Copenhagen, 1751, vol. I, p. 460.

"Concerning Harvey's treatise, I too have, till now, been of the same opinion as your anatomists. For, in our art, and especially when we are dealing with the connection of the parts of the human body, we must have hands provided with eyes if we only are to believe what we see."[2]

If Svabe's friend Conring, had some early doubts, they were certainly overcome, for a decade later, he shows himself an ardent advocate of the new doctrine.[3] Of Svabe's later beliefs in the doctrine we do not know, since no writings of his own have been left.[4]

Worm's Opposition

Worm took up the question for further discussion in a dissertation in 1632. This work in a very characteristic way reveals how absurd the doctrine of the circulation was to a man who had grown up with the dogmas of *Galen*.

Harvey's doctrine lacked the last link, not giving satisfactory explanation of the passage of the blood from the arteries into the veins; since he knew nothing of capillaries, he was forced to hypothesize a passage either directly through anastomoses or indirectly through porosities in the flesh or both ways. Worm immediately observed the weak point. How, he asks, is the blood to be transmitted through porosities, since it will coagulate as soon as it is outside the vessels? Since the flesh receives only that amount of blood necessary for nutrition, it would seem quite unreasonable that the entire volume of blood would ooze out of the vessels. Furthermore, how, in the pulmonary circulation, is the viscous venous blood to come out of the narrow-mouthed veins, and then force its way into the lung parenchyma, percolate through this, and again be sucked up by the smallest of the arteries? Harvey maintains that the blood leaves the heart through the arteries and returns through the veins. Worm asks for a reason for this and numerous other questions based on all the medical experience before him and his own theoretical beliefs which he does not think can be answered by Harvey's dogma. It is impossible, he concludes, to enumerate all the absurdities arising from the new doctrine turning both theory and practice upside down. The circulation of the blood is inconceivable. The entire volume of blood cannot pass

[2] *Ibid.*, p. 461.

[3] Conring, Hermann. *De sanguinis generatione et motu naturali opus novum.* Helmstadt, 1643.

[4] Ingerslev, V. *Danmarks Læger og Lægevæsen fra de ældste Tider indtil Aar* 1800. Copenhagen, 1873. Vol. I, p. 521.

through the heart because the entire volume does not come to the heart. Worm's logic is indisputable; only his premises are wrong.[5]

BARTHOLIN'S "ANATOMY"

The most outstanding of Harvey's supporters was Jan de Wale (1604-49), who from 1633 was professor at Leyden. He did not confine himself to reading Harvey's doctrine but, about 1640, performed a series of vivisections, convincing himself of the truth of the doctrine. Some of his experiments were attended by a Danish student, Thomas Bartholin (1616-80) whom circumstances soon compelled to form an opinion of the new doctrine. In 1611 his father, Caspar Bartholin the Elder, had published a very popular text of anatomy, *Anatomicae institutiones corporis humani utriusque sexus.* He was now being asked to issue a new illustrated edition which would include the new discoveries, especially Aselli's discovery of the lacteals and Harvey's doctrine of the circulation of the blood. Walaeus offered to superintend the work but Bartholin wished to ask the opinion of his friend and uncle, Oluff Worm. In March of 1640 he gave an account of the matter to Worm. Thomas considered it important that he take the assignment since it was partly because of Walaeus' influence that he was asked and because of his interest in doing justice to his father's book. He also wrote that he had collected data on the new discoveries: "But certainly," Bartholin writes, "I am not going to do anything without your approval."[6]

Worm responded on 18 April 1640 that he encouraged the task; concerning Harvey, however, he still had great doubt. But he did not intend to dissuade Bartholin from including Harvey's doctrine but held that he must include the objection of Primrose too.[7]

Before Bartholin had received the letter he himself had his doubts about explaining the new theory. He wrote Worm in April 1640 that he had given the revision to the printer with some of the new discoveries included, but, concerning the circulation and the lacteals, he had left that subject to Walaeus, and they had agreed that Walaeus was to write two letters, one on the circulation and

[5] Worm, Oluff. *Controversiarum medicarum exercitatio sexta.* Copenhagen, 1632, Controversia V.

[6] *Epistolae Wormii,* vol. II, p. 665.

[7] James Primrose (died 1659), physician at Hull, one of the earliest opponents of Harvey. His *Exercitationes et animadversiones in librum Guilielmi Harveii de motu cordis et circulatione sanguinis* was published for the first time in 1630. It is included as a supplement in William Harvey's *De motu cordis et sanguinis in animalibus . . . cum refutationibus Aemylii Parisani et Jacobi Primirosii.* Leyden, 1639.

one on the lacteals which were to be included as supplements to the book.[8] This pleased Worm since it did not expose his ward to blame; however, he still had his doubts and wrote Bartholin that he was not yet moved to believe in Harvey's discovery although he was eager to read what Walaeus had written.[9]

Published in 1641, Bartholin's "Anatomy" was the first textbook containing an exposition of Harvey's doctrine. It sold well. Beyond all doubt, Bartholin himself believed in the principle of Harvey's doctrine, although keeping aloof from a single point to be mentioned below. During the following years we shall see him as an advocate of the circulation theory.

In the autumn of 1641 Bartholin had visited Montpellier and writes to Worm: "At a public debate, I there sharply defended the new doctrines on the lacteals and the circulation, against a professor. At first it made me hated by these persistent followers of Galen but later on I was feared because the debate was held with open doors."[10]

Half a year later the young Bartholin wrote to the master himself—Harvey. He speaks of Harvey's *inventa ingeniosissima* which will preserve his name until eternity. In opening his letter, Bartholin says that from his writings it is evident how much he appreciates Harvey and his achievement. He assures him that everywhere he has been a spokesman for the new doctrine. There are, however, some obscure points about which he would like to ask Harvey. Walaeus had explained, in the supplement, that varicose veins, owing to their weakness, could not bring the blood forward. The question Bartholin wants to ask is why these serous humors, ever and ever returning to the heart, do not produce palpitations. In the next place he would like to debate Scribonius Largus' theory of the ligature in blood-letting.[11] Finally, he would like to know if the veins do not move at all.[12]

If Bartholin imagined that this letter was to be an introduction to a lively discussion with Harvey, he was disappointed, for Harvey never answered the letter with these strange and obsolete questions. Bartholin, however, did not lose courage. In 1643, he raised his voice against the learned Fortunio Liceto (1577-1657—latinized Licetus) of Bologna, who had proposed his own theory of the circu-

[8] *Epistolae Wormii*, vol. II, p. 667.
[9] *Ibid.*, p. 670. See also Bartholin, Thomas. *Epistolarum medicinalium à doctis vel ad doctos scriptarum, centuria I & II.* (= *Epistolae Bartholini*). Copenhagen, 1663, p. 16.
[10] *Ibid.*, p. 685.
[11] Scribonius Largus was a Roman physician living at the time of Claudius (41-54).
[12] *Epistolae Bartholini*, p. 105.

lation, saying that blood travelled in two circles and was transmitted from the right to the left ventricle via the coronary veins.[13] Bartholin wrote to Worm relating this debate; but Worm's reply indicates the great want of judgment Harvey's followers had to combat. In fact, Worm is more pleased with the fancy of Licetus than with the doctrine of Harvey. He feels that Licetus' doctrine will be proved by anatomy and autopsy and that all the difficulties of the Harveian doctrine are avoided in his learning.[14]

This was too much for Bartholin. He gently tries to correct the old man's views, telling him that the circulation can only be proved by animal experiments. His letter runs: "I do not see how Licetus can defend his doctrine of the circulation, destitute of experiments as it is and only supported by arguments." He goes on to explain the reasons for the differences in structure of arteries and veins, and that in coursing from artery to vein, the blood is changed.[15]

THE PORES OF THE SEPTUM

Having thus seen Bartholin as an advocate of the circulation, we are apt to be a little surprised in learning that he still kept aloof from Harvey on one important point—that of the pores of the interventricular septum.[16] Harvey had once and for all denied the existence of such pores, but Bartholin still believed that blood was capable of oozing through the septum.

His personal opinion was expressed in 1648 when, in an apologia directed against Caspar Hofmann, who also denied the existence of such pores and maintained the blood passed through the lungs, he wrote: "I never denied that the blood is running through the lungs, but I hold that some part of the most subtile blood passes through the septum."[17] He goes on to explain that this is used in part for nutrition of the septum, but more important, the blood would evaporate if it were to go "the long and dangerous way through the lungs."[18]

According to the old doctrine, the more subtile part of the blood was needed for the formation of the vital spirit. Since the

13 *Epistolae Wormii*, vol. II, p. 713. *Epistolae Bartholini*, p. 125. The debate between Bartholin and Licetus may be found in two other letters: *Epistolae Bartholini*, pp. 143 & 149. Licetus published his doctrine in the treatise, *De motu sanguinis*, Utini, 1647.

14 *Ibid.*, p. 714. *Epistolae Bartholini*, p. 173.

15 *Epistolae Wormii*. Vol. II, p. 716. *Epistolae Bartholoni*, p. 176.

16 Gotfredsen, Edv. *Oldtidens Lære om Hjerte, Kar og Puls*. Disputatio. Copenhagen, 1942.

17 Bartholin, Thomas. *Vindiciarum anatomicarum pro parente quarta*. Copenhagen, 1648, p. 77.

18 *Ibid.*

physiology of respiration was unknown, it was believed arterial blood obtained its color by means of the vital spirit and innate heat of the heart. Harvey believed this,[19] and here Bartholin agreed with him.

Niels Steensen

The discovery that the heart is nothing but a muscle is due to Bartholin's disciple Niels Steensen. In April, 1663, he wrote Bartholin and told him of the results of his experiments with the heart which, he considered, proved the heart is a true muscle.[20] Bartholin replied with high praise but also with doubt. A true muscle it cannot be, he said, because Galen described a muscle as an organ of voluntary movement which the heart certainly is not.[21]

Steensen did not share this veneration of Galen. In his final work on this topic he said the heart is a muscle due to the experiments he had performed and furthermore "cannot be of special substance, nor seat of special substances such as fire, innate heat, or the soul; nor can it be the generator of certain humours such as the blood; nor the producer of certain spirits, e.g. the vital spirit."[22]

The Later Editions of Bartholin's "Anatomy"

After becoming professor, Bartholin published two editions of the "Anatomy" under his own name (1651 and 1673). The letters of Walaeus still accompany these editions, but the doctrine of circulation is taken up in the text which, in the 1673 edition, was much altered. Yet Bartholin is still doubtful about the heart being a simple muscle, and the doctrine of a permeable septum is faithfully retained.

Despite these divergencies, however, Bartholin was the man who introduced Harvey's doctrine to Denmark and also followed its development.

In 1661 Malpighi discovered the capillaries in the lung of the frog (*De pulmonibus*). Two years later Bartholin published a treatise, *De pulmonum substantia et motu diatribe,* acknowledging and supporting this view as the answer to how the veins and arteries were connected. Bartholin mentions this and Richard

[19] Harvey, William. *Exercitatio anatomica de motu cordis et sanguinis in animalibus.* Frankfurt, 1628, p. 42.

[20] *Epistolae Bartholini, centuria IV* (Copenhagen, 1667), p. 417. Steno, Nicolaus. *Opera philosophica.* Vilhelm Maar, Ed. Copenhagen, 1910. vol. I, p. 158.

[21] *Epistolae Bartholini, centuria IV,* p. 422.

[22] Steno (note 20), vol. I, p. 181.

Lower's observation of an alteration of the blood in the lungs.[23] He even hints at the physiology, saying that this pulmonary circulation serves to make the imperfect blood perfect either by excretion of something useless or by adding air to the blood.

CASPAR BARTHOLIN THE YOUNGER

Thomas Bartholin's son corrected the errors of his father. Caspar Bartholin the Younger succeeded to his chair in 1677. His first dissertation, *Positiones anatomicae,* read in 1678, maintained that all the blood passes through the lungs, therefore indirectly stating the septum is impermeable. He also explained more fully and exactly the pulmonary circulation. Two months later, he read a new dissertation, *De corporis humani oeconomia,* in which he gave a correct exposition of the circulation but said nothing of the septum. In the *corollaria,* however, which accompanied the dissertation, he says: "In my opinion everybody who states that the septum is permeable, has doubtless forced his way by means of a sharp stiletto, as such a way withstands the physiology of the body, and also seems contrary to reason."

In a later thesis, *De cordis structura et motu* (1678), Caspar did justice to Steensen's work. But not until his father died did he utter the naughty words, "Septum impervium est." This appeared in a treatise, *Speciminis historiae anatomicae corporis humani disputatio secunda* (1688).

Caspar Bartholin was indeed very interested in the doctrine of circulation and in 1707, in his *De circulatione sanguinis,* produced a schematic figure of the circulation. In his explanation of the figure he mentions the capillaries, *vasa capillaria,* as the transit point between arteries and veins.

Thus Thomas Bartholin may be said to have introduced Harvey's doctrine into Denmark although with some errors. The errors were corrected by his son, Caspar the Younger, while his pupil, Steensen, corrected one of Harvey's errors in stating the heart is nothing but a muscle.

[23] Lower, Richard. *Tractatus de corde item de motu et colore sanguinis et chyli in eum transitu.* London, 1669, p. 166.

Harvey, Descartes, and Young Olaus Rudbeck

STEN LINDROTH*

OLAUS RUDBECK'S *Nova exercitatio anatomica, exhibens ductus hepaticos aquosos* was published in the summer of 1653. In this work Rudbeck, then a young student in Uppsala, announced his discovery of the lymphatic vessels. Much has been written about this publication, not least about the bitter controversy which ensued with Thomas Bartholin on the priority of the discovery. This work on the lymphatic vessels, however, was not Rudbeck's first publication. One year earlier, when he was twenty-two years old, he had published and defended an academic dissertation entitled *De circulatione sanguinis* (May, 1652).[1] In this work he introduced Harvey and the doctrine of the circulation of blood into Swedish medical literature. Rudbeck's intention was clear: he wished to proceed cautiously and first acquaint Swedish academicians with the new revolutionary physiology before he presented his own remarkable discoveries.

Understandably enough, the dissertation *De circulatione sanguinis* has lain eclipsed by the greater work which came after it and for this reason has never been closely studied.[2] Moreover, it is, of course, essentially unoriginal work, dependent upon the Harveian literature. Study of it shows, however, that the young cub was stretching his claws even in his very first work. The dissertation in itself has much of interest, and it throws light on the intellectual milieu in Sweden in which the young Rudbeck developed—with Harvey as one of his lodestars.

Rudbeck dedicated his dissertation to Queen Christina. A month before, in April, he had demonstrated before her his discovery of the lymphatic vessels. Now he applies to the queen the theme of the new physiology: she is Sweden's heart, *Cor Sveciae*, from which life-giving power, like the blood in the arteries, travels through the body of the state, and to which it all returns. Then in twenty-six theses, most of them brief, Rudbeck proceeds to takes up his subject. He begins with some general propositions: the

* Uppsala, Sweden.

1 *Disputatio anatomica de circulatione sanguinis* . . . praeside Olao Stenio. Arosiae, 1652.

2 It is mentioned in passing by Bayon, H. P. William Harvey, physician and biologist: his precursors, opponents, and successors. *Ann. Sci.*, 1938, *3*, 95. Also *cf.* Hedenius, P. *Om upptäckten av blodomloppet*, V-VI. Uppsala, 1892. pp. 61-65.

macrocosmic circulation of water as a parallel to that of the blood in the body, the circular course followed by the blood as a violent movement (*motus violentus*), the beat of the heart as its driving force. Then follows an analysis of the way the living heart works: of the movements of the auricles and the path of the blood from the vena cava and the pulmonary vein into them, of the ventricles, of the heart's diastole and systole, of the rôle of the cardiac valves, and the expulsion of blood into the aorta. Rudbeck refers to animal experiments and in general draws upon abundant empirical and experimental evidence, especially ligatures of the large vessels. Particular attention is then given to the arterial pulse. In a series of demonstrations he determines that the arteries pulse neither by their own power nor by any *facultas pulsandi* derived from the heart through the walls of the arteries; the pulse is simply a result of the blood's being driven by powerful force at every beat of the heart through the arterial system. Then follows quantitative data. Rudbeck assumes that twenty pounds of blood per hour passes through the aorta (4,000 beats x ½ drachm). The blood, he maintains, then goes from the arteries over into the veins; experiments demonstrate that numerous connections (anastomoses) must exist between the arteries and veins. The contrary direction taken by the blood stream in the two types of vessels, *i.e.*, to or from the heart, is shown by ligatures (and the structure of the valves in the veins). Rudbeck devotes the last paragraphs of his dissertation to a detailed topographical description of the arterial system of the body for the purpose of showing how the arteries always are accompanied by veins and are joined with them by anastomoses. In the scholastic custom of *quaestiones,* three questions conclude the brief dissertation. I shall return to one of them.

Did Rudbeck in any way contribute to knowledge of the circulation of the blood? Does his work have any independent value? The questions can be answered only after a careful examination of the literature which then existed on the subject, that is, the writings of Harvey and his successors. Not until quite late, the beginning of the 1640's, did Harvey's sensational discoveries begin at last to gain ground on the continent, and even around 1650 the victory was still not assured. But several men, especially in Cartesian Holland, had nevertheless picked up Harvey's ideas and made them their own and defended them. It is against the background of this early Harveian tradition that Rudbeck's dissertation must be appraised.

Rudbeck himself refers to three writers: Harvey, Walaeus, and Conring (also, in a special connection, Sanctorius). On the other hand, he never cites the work which has actually been his main source, namely, the *Anatomia reformata* (1651) of the famous Danish anatomist Thomas Bartholin. Bartholin, with whom Rudbeck was later to have so much to do, had been introduced to Harvey's doctrines during his student years in Leyden around 1640 under the expert supervision of Johannes Walaeus. But in the first two editions of his anatomical textbook (1641, 1645) he did not dare include such doctrines, however convinced he himself was of their truth; instead he inserted two letters, soon famous, from his teacher Walaeus on chyle and the circulation of blood. It was not until the third, rather extensive, revision of the anatomy, the *Anatomia reformata* of 1651, that Bartholin presented a detailed and excellent account of the circulation of the blood, filled with facts, evidence, and demonstrations. Bartholin was nevertheless not a thorough radical; as long as he lived he clung to the Galenic belief in the existence of pores in the septum of the heart and in the task of the left ventricle of producing the "vital spirits" (spiritus vitalis).[3] Rudbeck has taken a good deal of information and arguments from Bartholin's book, for the most part basic ideas, ultimately, of course, deriving from Harvey himself, on the movements of the heart and the direction of the blood stream in the vessels.[4] But Rudbeck seldom copies word for word; with great freedom he has taken what had suited him, reorganized and recast it. His dependence upon the older Danish anatomist was natural enough. Bartholin's textbook (originally a reworking of his father Caspar Bartholin's *Institutiones anatomicae)* enjoyed much popularity at that time. Rudbeck must have come upon a copy of the new edition very quickly, for it was published only nine or ten months before his own dissertation.

Bartholin has also supplied Rudbeck with illustrations. In the latter's *De circulatione sanguinis* there is a copper engraving drawn by Rudbeck himself and engraved by an older student, Magnus Celsius,[5] later to become professor of mathematics and

[3] Bartholin, Thomas. *Anatomia . . . tertium ad sanguinis circulationem reformata.* Leyden, 1651. pp. 232-71, 402-48. *Cf.* Gotfredsen, E. The reception of Harvey's doctrine in Denmark. *Acta med. scand.*, 1952, *Suppl. 266,* 78-83.

[4] A detailed comparison would take us too far here. Rudbeck follows Bartholin more or less closely, especially in theses 6-13, partly in 14, again in 15, the beginning of 16, and in 17-18.

[5] The engraving is signed "MNH," *i.e.*, Magnus Nicolai Helsingius, as Celsius called himself at that time. *Cf.* Rudbeck, J. Bibliografiska anteckningar om Olof Rudbeck den äldres anatomiska skrifter. *Samlaren,* 1904, 38; also (by the same author) *Bibliotheca Rudbeckiana.* Stockholm, 1918, p. 188.

founder of the famous scholarly family of Celsius. This plate shows two hearts, one in systole, the other in diastole; both have been taken from Bartholin's *Anatomia.* In addition, two more objects are pictured: a man's shaven head, which is said to demonstrate the veins and their anastomoses with the arteries; and two human legs with veins, the one with ligatures and decided swellings at the valves in the veins. The first of these two figures Rudbeck seems to have taken from a Dutch broadside representing a "bloodletting man" which had happened to fall into his hands.[6] But he has searched other sources also for material for his illustrations. In the Waller collection in the library of Uppsala University there are preserved two copies of Rudbeck's dissertation into which have been pasted a colored drawing done by hand of a human left arm with ligatures and swollen veins. One of these plates, on parchment, has been done by Rudbeck himself and carries an inscription by him; the other is a copy. The drawing of course is to be traced ultimately to the well-known figures in Harvey's own *De motu cordis,* but it is also to be found in Bartholin,[7] and Rudbeck has quite probably taken it from there.

However, Bartholin's *Anatomia reformata* cannot have given the Swedish anatomist his first inspiration, for the book was not even a year old when he wrote his dissertation. Rudbeck was a follower of Harvey, and he was evidently far advanced in his anatomical researches before Bartholin's book left the press. As early as 1650, when he was nineteen years old, we find him immersed in the new physiology: during the butchering of a calf he was studying the movements of the still living heart[8] and about the same time he first observed the lymphatic vessel by the liver. During the following year (1651) Rudbeck delved still more deeply into the problems of both the circulation of the blood and of the new lymphatic system, performed dissections and vivisections on dogs, calves, sheep, and cats, and applied ligatures on veins as well

[6] *Katoptron. Speculum venarum et arteriarum. Miroir des venes et arteres.* T'Amsterdam bij Rombout Vanden hoeje [n.d.]. I found this publication pasted in a copy of Caspar Bartholin's *Institutiones anatomicae, novis recentiorum opinionibus . . . auctae ab Thoma Bartholino* (Leyden, 1641) now owned by Uppsala University Library (Obr, 46:340). This copy was once in the possession of Isaacus Cujacius, Master of French in Uppsala, 1637-38; *cf.* Högberg, P. Le Genevois "Isaacus Cujacius" (Isaac de Cuyaux). *Revue d'histoire suisse,* 1926, *6,* 219. It is not certain, however, if Rudbeck had access to it; Cujacius seems not to have been residing in Sweden around 1650.

[7] Thomas Bartholin, *op. cit.,* p. 411.

[8] Rudbeck, Olaus. *Nova exercitatio anatomica, exhibens ductus hepaticos aquosos.* Arosiae, 1653. C iv.

as lymphatic vessels. In March 1652, he investigated anastomoses of veins and arteries in sheep.[9]

Rudbeck's first introduction to Harvey's work has thus been effected by writers other than Bartholin. Has he read the master's *De motu cordis?* The question is difficult to answer, for Harvey's arguments and his conclusions were adopted and developed by all his successors, Bartholin and others; Rudbeck can very well have become acquainted with them there. However, direct traces of Harvey's work may be found in his dissertation. The comparison in the dedication between the Swedish queen and the living heart is probably inspired by Harvey's dedication to Charles I, "the heart of the republic." Rudbeck seems also to have taken from Harvey the pharmacological point of view: only circulation of the blood can explain how externally applied drugs can be felt in the whole body.[10] And in his treatment of the arterial pulse as a passive occurrence, caused only by the violent expulsion of blood into the aorta, he has obviously borrowed arguments directly from Harvey: those dealing with the reception of blood in an aneurysm of the neck[11] and with the similarity of the structure of arteries and veins in certain animals.[12] But far more important to Rudbeck's theses has clearly been Johannes Walaeus' above-mentioned work, *Epistolae duae de motu chyli et sanguinis* (1641), which he could already have studied in the earlier editions of Bartholin's anatomical textbook. Walaeus, the foremost of Harvey's early protagonists, had here supplemented the master's doctrine of the circulation of the blood at several points by ingeniously devised experiments and had lent it additional strength; Harvey and Walaeus were frequently named in the same breath by the rising generation as the mainstays of the new theory. Rudbeck has borrowed much directly from the Leyden professor's first and more important letter: how an artificial venous pulse can be induced, how the emptied crural vein of a dead animal can be filled by compressing the corresponding artery, etc.[13]

[9] *Ibid.*, D 2*v*. *Cf.* Hofsten, N. von. Upptäckten av bröstgången och lymfkärlssystemet. *Lychnos*, 1939, 265-6.

[10] *Op. cit.* (note 1), thesis 26. *Cf.* Harvey. *Exercitatio anatomica de motu cordis.* Frankfurt, 1628, p. 61. (Facsimile ed., *Monumenta medica*, V. Florence, 1928.) The argument is also to be found in Conring, Hermann. *De sanguinis generatione et motu naturali.* Leyden and Amsterdam, 1646, p. 402.

[11] Rudbeck, *op. cit.*, thesis 14; Harvey, *op. cit.*, p. 25.

[12] Rudbeck, *op. cit.*, thesis 14; Harvey, *op. cit.*, p. 14.

[13] Rudbeck, *op. cit.*, theses 14 and 17. *Cf.* Walaeus, Johannes. *Epistolae duae de motu chyli et sanguinis*, in Thomas Bartholin, *op. cit.* (1651), pp. 551 and 538.

Rudbeck's dissertation in its essentials can thus be traced back to the available literature on the subject; nor is this surprising. He has read Walaeus and Harvey and, in addition, shortly before composing his disputation, Bartholin's revised handbook. Yet there is more to be said. His *De circulatione sanguinis* also contains independent contributions, and it has its own tone. The work gives ample testimony of having been composed by an enthusiastic young anatomist who, scalpel in hand, has himself probed into the mysteries of the living body.

What he has to add beyond his predecessors is not much, perhaps, but it is by no means unimportant. Rudbeck inserts a general comment or here and there adduces quantitative values, etc., which seem based on new experimental data; the computation of the quantity of blood expelled into the aorta is, for example, his own. Special interest is to be attached to his careful account of the arterial pulse. It has, of course, been in the main adopted from Harvey but it contains new insights and turns on a point emphatically at variance with the preceding literature. Harvey as well as his successors insisted that the arterial pulse was simultaneous, that it occurred *at one time* at all points in the arterial system. This Rudbeck denies (most likely upon theoretical grounds): the arteries do not pulse simultaneously, but the farther the artery lies from the heart, the later does the pulse occur.[14] In this fashion he answered a criticism that the Galenists could direct against Harvey's theory of the arterial pulse as a passive phenomenon. That this pulse was present everywhere at the same time showed rather that it depended upon an active dilatation of the arterial wall; for how could the blood reach in one moment the vessel's outermost branches?[15] The Harveians involved themselves at this point in difficulties—which Rudbeck thus overcame by a new interpretation of the mechanics of the pulse.

Just as remarkable is the interest which Rudbeck devotes to the question of connections between arteries and veins. It was one of the crucial and most controversial points to have arisen in the subject. Harvey's critics had not neglected to point it out as the weakest link in his chain of evidence: how the blood was trans-

[14] *Op. cit.*, thesis 14: Omnes arterias simul non pulsare, sed a corde remotiores tardius. In contrast, Harvey, *op. cit.*, p. 25: nec est expectandum, ut propter motum sanguinis tempus inter constrictionem cordis, et arteriarum (praecipue magis distantium) dilationem intercedat, ne fiant simul.

[15] *Cf.* Conring, *op. cit.*, p. 373. Significantly enough, Walaeus and Bartholin were forced by their belief in the simultaneity of the pulse to come to the Galenic conclusion that it was induced not only by the cardiac systole but *also* by a certain "facultas" in the arteries; Walaeus, *op. cit.*, pp. 558-9, and Bartholin, *Anatomia reformata*, p. 439.

ferred from artery to vein was a mystery. Harvey himself was in doubt; he spoke of direct connections between the vessels (anastomoses) and of certain *porositates carnis,* porosities in the tissues through which the blood worked its way. Later, in his controversy with Jean Riolan (1649), he became much more inclined to doubt entirely the existence of direct anastomoses between the vessels.[16] His disciples took up various positions. As a rule they were willing to accept as an alternative a free movement through the tissues, but at times they were very unclear. A characteristic treatment of the question is to be found in Paul Marquart Slegel's *De sanguinis motu commentatio* (1650), in other respects a concise and lucid defence of Harvey. Slegel defines the term "anastomose" so vaguely that it appears to embrace every possible sort of communication between the two systems of vessels.[17] However, direct anastomoses were not in themselves any startlingly new concept; Galen, starting from an entirely different basis, had said much about them. They found a warm defender in Hermann Conring, who was one of Harvey's first disciples on German soil. In controversy with his master, Conring maintained (1643) that no blood passes through open tissue. The peripheral transfer from arteries to veins occurs only through anastomoses, *i.e.,* closed connections between the vessels (*vasi vas connectatur*); many persons had already seen and described them.[18] It is uncertain if Rudbeck had read Conring's richly informative but voluble and conjectural work. But he too is a confirmed believer in anastomoses, in the sense of direct connections between vessels. A central concern of Rudbeck's was to show how they were to be found throughout the body; to this topic he devoted the last theses of his dissertation. Naturally, no more than Conring and other anatomists at that time, had he ever seen any true "anastomoses"—it is the discussion of the principle of their existence that is of interest here.

Rudbeck's dissertation is utterly lacking in polemical tone. It was no concern of his to enter into controversy with Galenism; others had done that abundantly before him. He belonged to the second generation, who felt no burning need to free themselves of older doctrine—even in his first years as a student he had been an adherent of the new physiology. His dissertation therefore is concise, sober, and informative. Rudbeck is temperate in other re-

[16] *Cf.* Harvey. *Exercitationes anatomicae duae de circulatione sanguinis ad J. Riolanum.* Rotterdam, 1660, pp. 204-8.

[17] Slegel, Paul Marquart. *De sanguinis motu commentatio.* Hamburg, 1650. pp. 126-33.

[18] Conring, *op. cit.,* pp. 384-7.

spects also. He makes no attempt at a definitive interpretation of the phenomena or to explain their "causes." Like Harvey, he was saturated in the new scientific spirit and threw overboard all traditional philosophical jetsam. "Never," said his great Danish contemporary Niels Stensen, "can mistakes be more easily made than when one explains the function of an organ"; Rudbeck might well have uttered the same words. Therefore, what he does *not* include in his dissertation is also of considerable interest. His predecessors still adhered to certain particulars in Galenism or obscured the naked truth by speculative additions. Walaeus believed in the active dilatation of the arteries in the pulse; Descartes and his disciples considered the movements of the heart to be caused by boiling and dilution of the blood in the ventricles; Bartholin never abandoned his belief in the pores in the septum of the heart; Conring and George Ent veered into scholastic profundities.

In Rudbeck, however, all Galenic remnants have been weeded out. For this reason he shows himself to be much advanced. Special interest attaches itself to his attitude toward the old fundamental dogma of the "spirits" of the blood and of the body in general—for we soon see that it opens up significant perspectives.

As is well known, according to current Galenic physiology three kinds of "spirits" operated in the body: a *spiritus naturalis,* manufactured in the liver; a *spiritus vitalis,* produced in the left ventricle of the heart from blood exuding through the septum of the heart and air from the lungs; and a *spiritus animalis,* produced in the brain. With Harvey's discoveries this doctrine began to totter. Belief in special vital spirits in the blood of the arteries (*spiritus vitalis*) was especially undermined, for it could not be easily reconciled with the new view of the task of the left ventricle of the heart which came as a consequence of the circulation of the blood. Harvey himself, however, had certain difficulties with the "vital spirits" and could never get entirely away from them,[19] and the usually conservative Bartholin was convinced of their existence. But others accepted the consequences of the new teaching and launched an attack against the doctrine of *spiritus vitalis.* Conring was one of them.[20] And in a work perhaps too little noticed, *Dissertatio de corde* (1648), the Dutch physician Jacobus de Back directs a trenchant criticism on the basis of Harvey's discoveries

[19] Harvey discussed the question thoroughly in *Exercitationes anatomicae . . . ad J. Riolanum, ed. cit.,* pp. 225-33.

[20] Conring, *op. cit.,* pp. 336-41.

against the entire scholastic doctrine of *spiritus;* no invisible "spirits" need be assumed to explain the life processes.[21]

Rudbeck has categorically acknowledged his views of the matter. In one of the questions which conclude the dissertation, he replies that there is no "spirit" in the body except the animal *(spiritus animalis).*[22] He thus denies the existence of the two other "spirits," the "natural" and the "vital." Rudbeck's opinion was of course based upon his conviction of the truth of Harvey's doctrines. But another influence had also made itself felt. Rudbeck reveals himself here as a convinced Cartesian. It is well known how the philosopher René Descartes looked upon Harvey as a fellow rebel and in his revolutionary book, *Discours de la méthode* (1637), took up the fight for the circulation of the blood. It is true that he corrupted Harvey's doctrines through some unfortunate additions of his own, but it is of greater significance that in Cartesian circles the circulation of the blood was accepted as incontrovertible truth. On the continent of Europe the Cartesians were spreading not only the Copernican theories of the universe, but also the gospel of the new physiology. In *Les passions de l'âme* (1649) Descartes once more engaged in propaganda for his special form of Harvey's teachings. In this work it was incorporated in a larger physiological system, the cornerstone of which was the famous mechanistic theory of "spirits": of the various types of *spiritus* in Galenism Descartes rejected two and retained only the "animal spirits" (*spiritus animales*). These circulated from the brain through the nerves to the several parts of the body, where they set the muscles in motion.[23]

"Animal spirits" became in physiology—like the vortices of matter in cosmology—a kind of Cartesian theme song. On this point, therefore, Rudbeck is seen as an adherent of Descartes' ideas from the beginning of his scientific career. The question is, however, whether it is not in general inspired by Cartesian teachings.

The master himself, Descartes, died, as we know, in Stockholm in February, 1650, after some rather unhappy months as philoso-

21 Back, Jacobus de. *Dissertatio de corde,* ed. tertia. Rotterdam, 1660, pp. 64-78. *Cf.* p. 78: Concludo itaque, cum neque in toto corpore talis substantia, quae cum spirituum definitione convenit, reperiatur, neque quae alicui fini spiritibus attributo competat, nec spiritus esse ullos, nec in corde elaborari posse.

22 *Op. cit.,* Quaestiones: An detur aliquis spiritus in corpore praeter animalem? Negatur.

23 Descartes, René. *Les passions de l'âme.* In *Oeuvres de Descartes.* Adam and Tannery, Eds. Paris, 1909, pp. 331-42. *Cf.* Saint-Germain, B. de. *Descartes considéré comme physiologiste et comme médecin.* Paris, 1869, pp. 156-62; also Foster, M. *Lectures on the history of physiology.* Cambridge, 1924, pp. 258-66.

pher at the court of Christina. That his ideas were then familiar to a limited group of intellectuals at the court is evident. But it has always been uncertain if by this time they had spread to Swedish learned circles, and particularly if they had reached Uppsala University. In this connection one name has been mentioned: Olaus Stenius, from 1648 Professor of Medicine. According to an old though unauthenticated tradition he is supposed to have been Uppsala's first Cartesian.[24] Our investigation of Rudbeck's *De circulatione sanguinis* throws a clearer light on the question. For Stenius was Rudbeck's teacher, and it was he who presided at the young student's defence of his dissertation. And there is considerable reason to accord the fairly unknown Stenius a far greater significance in Rudbeck's development than he has previously been given. Rudbeck has been fondly regarded as a young genius who boldly carved out his own path; perhaps this is a somewhat exaggerated view to take. When in the spring of 1652 he made his famous demonstration of the lymphatic vessels before Queen Christina, it was not he but Olaus Stenius who lectured on the new findings—the teacher, thus, lent his authority to them and felt that he shared in his pupil's discoveries. Some years later Stenius presided at the defence of a dissertation entitled *De respiratione* (1655), which also treats the circulation of the blood as obvious truth.[25] For Stenius, Harvey's doctrine was clearly of fundamental significance. It seems reasonable to find signs of his guidance in the introduction of Rudbeck to the new physiology. In this connection his reputation as a Cartesian takes on new meaning. The teaching of the circulation of the blood was a part of Cartesian philosophy. Rudbeck's dissertation in 1652 was at one point certainly inspired by Descartes. Every indication points, therefore, to the conclusion that Rudbeck was trained as an anatomist under Cartesian influences introduced by his teacher Stenius. Descartes' writings had shown both of them the way to the new theory of the circulation of the blood; in Harvey, in Walaeus, and in Bartholin a more thorough understanding could then be secured.

The question of how Olaus Stenius became acquainted with Cartesian philosophy then remains. He had evidently never studied abroad. It is most likely that he had associations with the

[24] *Cf.* Annerstedt, C. *Upsala universitets historia,* Uppsala, 1908, II:1, pp. 92-3.

[25] *Positiones anatomicae de respiratione,* resp. Ericus Olavi Flaeckerus. Uppsala, 1655, B 3r and B 4r.

intellectuals at court in the capital, possibly during the time Descartes himself resided there. Of special interest then is the queen's personal physician, the Frenchman Grégoire François Durietz, who seems at an early enough date, through the offices of the French ambassador Chanut, to have been initiated into Descartes' ideas; when the philosopher himself came to Stockholm, Durietz became his good friend.[26] It is quite likely that Stenius knew Durietz well; possibly he studied with him some time in 1644.[27] It may be added that precisely that work in which Descartes had recently presented his physiological doctrines, *Les passions de l'âme,* was then especially well known in Stockholm. No later than 1648 Christina had a manuscript of the work in her possession, and two years later Descartes distributed copies of the freshly printed book in Stockholm.[28]

For Rudbeck, Harvey and Descartes became a foundation, a starting point. In his dissertation *De circulatione sanguinis* he had demonstrated what he had learned from them and from others. By it, he introduced to Sweden, at that time still rather backward in scientific matters, the modern experimental method of scientific research. Only after this did he find the time ripe to carry the revolution in physiology further and announce his discovery of the lymphatic vessels—the first independent Swedish contribution to the history of natural science.

[26] Baillet, Adrien. *Vie de Monsieur Descartes.* Paris, 1946, p. 266 ("du Ryer. . . . son ami particulier"). *Cf.* Adam, Ch. *Vie & oeuvres de Descartes.* Paris, 1910, p. 522.

[27] *Cf.* Lindroth, S. *Paracelsismen i Sverige till 1600-talets mitt.* Uppsala, 1943, p. 389.

[28] Adam, *op. cit.,* p. 489.

Harvey in the History of Scientific Thought

PEDRO LAIN ENTRALGO*

TO understand clearly the historical meaning of Harvey's physiology, we can do no better than to place ourselves at the starting point of modern medicine, within the circle influenced by the Italian Renaissance, that is, beginning with the *Fabrica* of Vesalius. Modern physiology may be conceived as endowing with inner localized [mechanical] motion the cadaver described by Vesalius.

The physiological motions described in *De motu cordis* and in the two Disquisitions to Riolan are those of a cadaver which begins to breathe and whose heart is beating again. Let us imagine this reality and let us make an attempt to understand what Galen, the greatest authority of ancient physiology, and Harvey, the initiator of modern physiology, have to say about it. We shall analyse in turn the different ideas each had of the reality they were observing and the different methods used by each in attaining the knowledge of this reality.

1. *The reality.* It is a body that breathes and whose heart beats. What does Galen think of this reality? How does Harvey interpret the same reality? I am not referring here to their respective doctrines, right or wrong, as to the actual mechanics of this breathing human body, but to the theories they postulated to explain the very existence of these phenomena.

Galen's physiology rests on two fundamental principles: substance and force. The physiological processes of Galen are always transformations of substances. To digest, for instance, is substantially to transform food into blood; an organ nourishes itself by transforming blood into its own tissues; when a muscle contracts it is because it has been infused with certain substantial agents that incite it to contract; venous blood becomes arterial blood by the addition of the vital spirits, elaborated in the lungs and in the heart. Life, in short, is a continuous transformation of substances.

Physiological motion, moreover, is always the actualization of a specific "force". That is to say, the parts of the animal are set in motion by their own and specific activity, which is hardly ever the result of a "passive" reaction. The stomach "attracts" the food; the heart and the arteries "pulsate" actively; the kidney "sucks"

* Madrid, Spain.

the blood in order to purify it, and the liver as well as the right ventricle also "suck" the blood from the portal vein and from the vena cava respectively. Life is autonomous movement of the organism as a whole or of any of its component parts. The independent motion of such component parts is the key to the physiology of Galen and its danger because it implies an endless multiplicity of specific "forces" (*dynámeis*) and of the "spirits" which are their instruments (*pneumata*). Hence the scientific concept of the reality of a living body in repose according to Galen's thought; the blood moving in that body is being consumed and made anew continually; its motions are caused more by forces of attraction than by impulsion; that is, from venous blood it becomes arterial blood by a substantial transformation.

Harvey's thought is quite different. It is true that in his Aristotelian mind (there is a great deal of the "ancient man" still in Harvey) the category of substance survives very strongly while modern science was beginning to doubt its validity. If one reads Harvey's writings carefully, one can perceive that he is trying to delay or even avoid facing the physiological problem of "substances," or that he attempts to solve it while ignoring its basic principles. When the venous blood becomes arterial blood it does not change into another substance: *"spirituosus sanguis, non minus sanguis est"* (*De motu cordis, Prooem.*); it goes through a process of disaggregation as it passes through the lungs; it does not become another substance but is the same substance in a different "physical state." To Harvey, as the blood moves about, the important thing is not so much that it might be generated anew but that it is displaced. Although he admits a moderate hematopoetic activity in the liver (still a prey to Galenism), the problem of such an activity is of minor consequence to him. Harvey is not concerned with the problem of how the blood is generated but as a "modern man" of how and why it moves in space.[1]

Let us see how Harvey conceived such movements of the blood. To Galen, the arteries, which are moved by the *dynamis sphygmiké* sent by the heart throughout the length of the arterial tunic, dilate and contract actively; they are living organs even though the impulse to induce such movements comes from the outside, that is from the heart. To Harvey, on the other hand, the arteries are

1 The investigators belonging to the German school of modern physiology (Paracelsus, van Helmont, etc.) interpret as "substantial" the physiological motions, although in a way quite different from Galenic thought. This should be kept in mind as relevent to the well-known hostility of Paracelsus to the science of anatomy.

merely elastic pipes, dilated by a liquid wave, like the fingers of a glove, to use his own comparison. The "force" of the old *vis pulsifica* is no longer *vis* but simply mechanical "impulsion," and the motions of both the blood and its containing vessel are merely the passive reaction of a blow: *"pulsum, quem nos sentimus in arteriis, nil nisi sanguinis a corde impulsus est"* (*De motu cordis,* V). What makes the artery a moving organ is not a specific "property," not some dynamic force peculiar to the "living form" of the animal but the artery's own elasticity. And this is what he means when—still employing by verbal inertia the language of his day—he uses the expression *vis* or *facultas pulsifica.* For instance, he is talking of this natural elasticity of the arteries when he says that "the medial ligatures do not stop the *vis pulsifica* from extending beyond such ligatures to propel the blood toward the furthest extremities of the body" (*De motu cordis,* XI).[2]

Let us now consider the specific function of the heart, where the hydrodynamic impulse that dilates the arteries originates. In Galen's physiology the heart is a living organ, the center of animal heat, the source whence the *vis pulsifica* originates, and the agent which brings about the transformation of substances: air into the vital spirits, venous blood into arterial blood. In Harvey's physiology, the heart is simply a hydraulic pump that draws the blood from the vena cava to propel it into the arteries: *"deglutitio quaedam est* [*scil. motio cordis*], *et transfusio sanguinis e venis in arterias" (De motu cordis, V).* It is not by accident that Harvey compares the mechanics of cardiac revolution to the mechanism of fire arms: *"mechanicum artificium quod scopletis adaptant."* Harvey's concept of the function of the heart is quite definite: "the only act of the heart (*una actio cordis*) is to transfuse the blood and to propel it towards the farthest extremities of the body, via the arteries" (*ibidem*). He does consider that the heart, besides displacing, moving locally, and distributing the blood, "may add to the blood some elements, heat, spirit, or perfection"; but to clarify this point will require new observations, and Harvey thinks that it better be left for another time, *"posterius inquirendum est."*

How does the heart propel the blood? Or, in other words, how and why does the heart contract? Harvey admits a *facultas pulsifica* in the walls of the heart: *"unde auricula dilatata, sua facultate se*

[2] One may read the same thing in the *Exercitatio anatomica altera ad Riolanum: "pulsificam vim non per arteriarum tunicas a corde manare."* He also speaks later on of a *facultas pulsifica* of the auricle to which I shall refer very soon.

contrahens" (found in the second *Exercitatio anatomica ad Riolanum*). But this ability to pulsate is not a specific virtue, it is simply muscular contraction, because the heart is a muscle and its movements should be attributed, in agreement with Aristotle, *ad contractile,* "to the contractile element," be it in nerve or in muscle (*De motu cordis,* XVII).[3] It is in this generic sense that the passage from Harvey quoted above must be interpreted: "sua facultate pulsifica," its faculty to pulsate; that is, the ability of auricle and ventricle to contract because they are muscle.

The problem of the heart's contractions is thus to be studied as muscular contraction which is the origin and foundation of all other possible local motion. Harvey feels the need to study this problem but again he postpones it to another time: "This problem should be clarified, I believe, if at some time I could speak of the moving organs of the animal and of the fabric of the muscles on the basis of my own observations" (*ibidem*). However, in the recesses of Harvey's mind lurks a fundamental question that is hard to ignore. How does the activity of the wall of the heart begin? What is the agent that sets to action the heart's contracting muscles? Descartes, who considered the physiological activity of the heart above all as the ability to dilate, was about to devise his well-known theory of thermomechanics. But the systolic movement is a contraction of the muscle, not a dilation of the cavities brought about by the pressure of the blood. How, then, does this contraction come about?

Harvey considers the blood itself as the possible agent. There is a passage in *De motu cordis* in which he seems to attribute to the blood an inherent vital activity, a kind of obscure spontaneous motion. In describing the final paralysis of the heart he says: "When the heart, including the right auricle, comes to rest from its pulsation, as in the hour of death, in the blood within this right auricle an obscure motion, an overflowing persists, clearly visible, and a kind of palpitation, as if the blood were impregnated by heat and spirit,"—*"tamdiu scilicet, qua calore et spiritu imbui videretur"* (*De motu cordis,* IV). This motion appears to be inherent to the blood as the auricle—Harvey insists on it—is at rest: *"et etiam auricula . . . quiescebat."* This interpretation becomes the more plausible as, a very few lines later, in a discussion on the *primum*

[3] Harvey calls attention to the fact that Aristotle calls *neura* "nerves," the muscular coats of the heart, because they are contractile since neuron would be derived from neúō, "new" or to contract.

movens, Harvey ventures the theory that the blood, and not the auricle, may be the first to pulsate, as it is the last to do so, according to his own observations, *"obscuram palpitationem post mortem retinere visus est."* And he recalls that, according to Aristotle, the sperm of all animals palpitates as it leaves the body, as if it were a living organism, *"velut animal quoddam."* Later on, Harvey in his second *Exercitatio ad Riolanum* writes again about a spontaneous motion of the blood, which he conceives as a kind of fermentation that could be the immediate cause of the auricular contraction: the blood from the vena cava, in proximity to the right auricle, warms up by virtue of its own internal heat *(ab interno suo calore)*, swells, and rises like a mass in fermentation *(turget et attollitur, fermentantium in modum)* and fills up the auricle which, dilating by the flow of such blood, then contracts by its own faculty to pulsate and injects the blood into the right ventricle. Here Harvey maintains that it is the heat inherent to the blood that warms up the heart and not the heart that warms up the venous blood, as he himself had repeatedly affirmed in *De motu cordis,* following the doctrine of Aristotle.

It is now time to take a synoptic view of Harvey's process. In *De motu cordis* he states that the heart possesses a natural primary property. Its heat is the *calidum naturale sive innatum* of ancient physiology. But twenty years later Harvey thinks that this heat results from that of the blood. Also it is the blood that incites the heart's contraction. The heart, which was considered first as a sort of internal animal generating heat *(animal quoddam internum),* becomes rapidly a hydraulic machine that requires outside heat and a stimulus to be set in motion. This was the course travelled by Harvey's mind.

And what are we to understand from this evolution in Harvey's thought? Harvey labors to reduce physiology to the changes in space of a body in motion. Thus if Vesalius' anatomy is or tries to be pure static architecture of the human body, Harvey's physiology is anatomical architecture in motion.

Let us compare now the relation between form and function as conceived by Galen and by Harvey. Form *(eīdos)*, action *(enérgeia)*, function or use *(khreía)*, and the final objective *(télos)* of each part of the organism are determined by the living being, are essentially related among themselves, are basically rooted in the "nature" peculiar to the living being, and consequently rooted in

"Nature": each part of the living being, according to Aristotle and Galen, has its own *katà physin,* its own peculiar ecstatic and dynamic function (its own motion), and works toward the accomplishment of its objectives, within the specific and individual objective of the whole animal. Consequently, there is no priority of form over function or vice versa; biological life is a circle, a snake that bites its own tail.

As modern physiology began, function was to be secondary to form. The contributions of Fabricius ab Acquapendente, Santorio, and Harvey cannot be understood except in the light of Vesalius' *Fabrica;* to each form its own motion. When Descartes attempts to explain to Beverwijk his theory of the motion of the heart, he says: *"Omnem motum sanguinis ex solo cordis calore ac vasorum conformatione deduxi."* To Descartes heat is the agent of motion; the shape of the vessels (calibre, valves, etc.) is the cause that determines the shape in space of such motion.[4] Harvey also takes into consideration "the symmetry and magnitude of the ventricles of the heart and of the afferent and efferent vessels" as well as "the harmonious and diligent artifice of the valves and fibres, and of the whole fabric of the heart" (*De motu cordis,* VIII). He doubts, however, that by looking at the structure of the dead body one can know the function of its parts. This is what the anatomists pretend who talk about the different parts of the animals and have looked at only one man, and a dead man at that: *"quid . . . unum tantum hominem, eumque mortuum introspiciunt"* (*De motu cordis,* VI). Form gives an indication as to function; but nature is much too complex for man to unravel it by *mentis agitatio,* no matter how subtle, and demands from the physiologist the direct examination of reality. Vivisection with comparative observation is the one and only way to travel from form to function, from anatomy to physiology. "No one can rightly determine the function or task of an organ without having studied, in different animals, its structure, location, its connecting vessels, and other accidents," says Harvey clearly in the preface to *De generatione animalium.*[5]

As Harvey's thought spreads out, it reveals first of all the effort of his genius to study the physiological processes as simple local motion; it is no accident that his work is contemporary to that

[4] Descartes, *Oeuvres,* Adam and Tannery, Eds., vol. IV, p. 4.

[5] Later on, Claude Bernard will write: "Concerning those organs about whose functions experimental physiology has told us nothing, anatomy is absolutely silent" (*La science expérimentale,* Paris, 1878, p. 105).

of Galileo. His thought also reveals the beginning of the intellectual tendencies of modern man—so magnificent and so shattering at the same time—to reduce to "secondary" or "consecutive" all the phenomena of Nature, to avoid carefully what Renouvier, the philosopher, calls *commencements absolus*. To the modern man of science nothing constitutes a beginning or a term of absolute reference.

Ancient man accepted easily the existence of realities or beginning phenomena. To Galen the motion of the blood in the arteries was secondary or consecutive to the *vis pulsifica* of the heart, but the latter is the first natural property which exists "because it does so." In the seventeenth century science is beginning to formulate the concept of "consecutiveness" and to reduce all natural phenomena to a category of relativity. Nevertheless, Descartes still thinks that all physiological motions are a consequence of a primary property, the *feu sans lumière* in the wall of the heart. The mind of Descartes, sitting on the fence between two epochs, still "needs" the existence of an "absolute beginning": the "fire without light" had been put directly in the heart by God, the creator of nature; and animals are born and will be born this way because God made them this way.

Harvey's experiments and speculations do lead him to realities and phenomena that to his understanding seem original causes. The pulse in the arteries is mechanical and a rational consequence to the pulse of the heart, which, in turn, is a rational consequence of its being a muscle. The heat in the heart does not originate in it but comes from the blood. But how to explain the rhythm in the contractions of the heart's muscle or the *calor internus* of the blood? Were these original properties put directly in the animal by *perfecta et divina Natura* who, according to Harvey himself, had put the heart in the body of the animal in the first place?[6] Perhaps; but Harvey is not satisfied. His state of mind was not that of Descartes when contemplating the theoretical reality of a fire in the heart. Every time Harvey is faced with any of these "absolute beginnings" his attitude is always the same; he will investigate the problem at another time. He postpones a definite judgment. William Harvey is a prey to the unassuaged thirst for knowledge, called "fáustica," the inability to accept the fact that there are things in the world because there are. After the dawn of the fifteenth

[6] Thus it is that Nature, perfect and divine, does nothing in vain, nor does it give a heart to an animal when it is not necessary. (*De motu cordis*, XVII.)

and sixteenth centuries, the morning of the modern world is fully ablaze.[7]

II. *The method.* The scientist will always develop his method according to his idea of the reality he observes. To operate methodically means to act with a predetermined purpose: be it real or imaginary, precise or vague. And a purpose is not possible without a definite idea of the reality to be studied. Galen's idea of reality determined the method he employed in physiology. As pointed out above, Galen observes, experiments, and describes scientifically. But, unavoidably, he selects and orients his observations, he plans and interprets his experiments, and he composes and writes his descriptions from his idea of the animal. Nature, to Galen, is the fountainhead of all activity, and its motions are intended for the accomplishment of certain ends inherent to it and decreed to the achievement of its own perfection. Each natural movement serves best its own appointed good and consequently the universal goal, as Nature always does what is best; the student of Nature, in short, can ascertain, by scrutinizing reality, the "how" and the "what for" of the creatures of nature. These are, briefly, the cardinal principles of Galen's interpretation and of his method. Heraclitus said that Nature delights in hiding. Galen, more optimistic, believed that the eyes of the student can penetrate into the secrets of the motions and purposes of the natural world. If Galen so often praises the occult wisdom and the happy order with which Nature accomplishes its ends, it is because he has perceived them with his own eyes. In the Galenic praise there is veneration, but also pride; Nature is truly admirable but it is to him *sophós,* to whom Nature has revealed her marvellous designs.

Harvey's original attitude toward Nature, on the other hand, betrays insecurity and lack of confidence. He is in agreement with Descartes. "When Descartes says that all things are doubtful [writes Zubiri[8]] he means ultimately that none of them offers, in its present aspect, a sufficient guarantee of strength upon which the human spirit can rest." Neither does Harvey find enough guarantee of strength to solve his own personal problem: traditional wisdom was not a firm foundation upon which to base a knowledge of the

[7] Harvey admits of course the existence of realities and of nature's originating causes. Expressions like "Nature makes," "Nature mixes," "Nature puts," etc. are frequent in his writings. (See what I shall say later about the "ancient" elements in his work.) But his "modern" tendency to search for an immediate cause in what he observes pushes further and further away such beginnings and "absolute" terms of reference. That is why he feels strongly the need to unmask solemn names and originating appearances: (*"detracta larva, evanescit, ut error, etiam admiratio." De generatione animalium,* LXXI.)

[8] *Naturaleza, Historia, Dios.* Madrid, 1944, p. 287

truth in Nature. One had to look for the answer in the *autopsy* of reality. But the world of Nature is obscure and complex. When Harvey decides to observe by himself the motions of the heart, his spirit vacillates, and he does not know what conclusions to reach by himself, nor what to believe from others (*De motu cordis,* I). At first everything is doubtful and confused. All is mystery. The expression, *arcana Naturae,* appears frequently in the revealing preface to *De generatione animalium,* and Harvey would readily accept the pessimistic words of old Heraclitus; "Nature delights in hiding"—*Impervestigabilis natura* in Harvey's words.

What is to be done in the face of such a dark and difficult problem? First, to find a method. The road to a knowledge of Nature is a long one; to find a safe way it will have to be a winding one, and an arduous one.[9] But the reward is gratifying, "In such work it is soothing not only to suffer fatigue, but absolute exhaustion," as Nature always "pays more than she has promised." Now Harvey only has to define his method.

As is often the case, Harvey developed his method in his youth but defined it only after middle age. As a young man Harvey made the many observations that led him to describe the circulation of the blood; as an old man, when writing the preface to *De generatione animalium,* he deems it necessary to meditate on the method he used as a naturalist; he wants to "propose for the students" the method which leads to "a safer path (than those once walked upon) to reach scientific knowledge."

All of those who believe that Harvey's method is a living example of Baconian induction—which means practically all who have written about Harvey—will be rather surprised to read from Harvey himself that his scientific method was that of Aristotle. "The ancient philosophers . . . instituted a way totally different (from simply commenting on what has been said) and by carrying out diverse experiments with indefatigable labors contributed to our studies a shining light." Drawing mainly from two texts of the Great Philosopher, the *Physica* and the *Analytica posteriora,* Harvey explains and makes his own Aristotilian doctrine from the *epagogé* (inductive method). The sure way to knowledge of nature consists in passing methodically from the particular to the uni-

9 "Nature herself [said Harvey to George Ent] is the most faithful interpreter of her own mysteries; what she shows in one species in an obscure and intricate way, she will show in another clearly and unmistakably." Thus the investigator has a long way to go; moreover, all one observes and the intricacy thereof makes it difficult to find words with which to express it. Much to his regret Harvey is compelled to coin new expressions, as happens to all men with original minds.

versal, from that which we perecive with the senses to that which we know through our intelligence; or, in Aristotle's own words, "to proceed from what to us is apprehensible and clear to what is most clear and apprehensible by its own nature" *(Physica,* 184 a). But it happens that in the transition from what one feels to what is universally understood there is a stretch of obscurity and indecision: "While in the act of seeing [says Harvey] the particular is clear and distinct; but should it be withdrawn from sight or should we close our eyes, that very thing, abstracted in phantasy or stored in memory, appears obscure and indistinct, and it is seen no more as particular but as something common and universal." This is why we must have recourse to our senses to judge rightly the ghosts that dwell in our minds. Without diligent and constant observation (in the most diverse circumstances and in as great a number as possible) there is no true universal knowledge. According to Aristotle the acquisition of knowledge proceeds as follows: from sensation there remains what has been sensed; from the permanence of what has been sensed comes memory; from multiple memory comes experience, from experience, universal reason, definitions, maxims, and common axioms. "No one can call himself prudent or wise [says Harvey] if he has not learned from his own experience, by virtue of a multiple memory and diligent observation, that things are as he sees them." The false students do not inquire as to what things are, but as to what others said about them, and proceeding afterwards according to false analogies they offer us verisimilitude for truth. Aristotle himself gives the absolute rule: when in doubt "one must believe more one's senses than one's reason. One must, of course, give credit to reason, but only when one's reason agrees with one's senses" *(De generatione animalium,* III, 10).

This is Harvey's method, and the way he understood induction. How did he put it in practice? Aristotle says that by "induction *(epagogé)* we discover the first principles" *(Analytica posteriora,* 100 b). What did Harvey understand by "first principles," how does he conceive them? I shall answer in advance; he conceives them as a modern empiricist, as an insecure man, lacking confidence in his intellectual powers in the face of nature and refusing to go beyond the experience of his senses. "All of modern philosophy sprouts, like a seed, from horror to error, to be misled, "a être dupe," wrote Ortega y Gasset.[10]

[10] "Kant." *Obras completas,* vol. IV, p. 28.

We could say the same of modern physiology. As we read *De motu cordis* we read about a Harvey full of mistrust who repeats as many times as he can, to excess, the "tests," be these straight observations reflecting on the anatomical structure, or be these experiments. His attitude is, *sit venia verbo,* that of a detective. He suspects everything, accumulates facts, and finally convicts nature of heathen acts: "the animal body has been convicted by its own confession that the blood circulates in it," Harvey seems to be saying. To Galen his experiments were a paean of nature; Harvey's experiments are an ingenious trap to force nature into admitting the truth.

Let us analyse, on the other hand, the nature of the "first principles" or "universal reasons" as postulated by Harvey by means of the inductive method. To those who do not believe in the circulation of the blood, because they fail to see both the practical reason for it or its cause, he answers: *"prius in confesso esse debet, quod sit, antequam propter quid, inquirendum."*[11] By means of the inductive method Harvey postulates not "whys" but generic "whats," universal facts; the most significant, as an example, is that the blood circulates in all animals endowed with a heart. It is to be pointed out that these universal facts are still strictly intuitive. Harvey does not practise yet the physiology of formal laws to be postulated later by Claude Bernard. He aims at a physiology of the senses. With figures he is trying to tell to our imagination and through it to our understanding what happens in the human body and how it happens. It is not until he writes *De generatione animalium,* in full maturity, that Harvey makes an effort to go scientifically and by induction from "facts" to "principles."

Nevertheless, there is something more than the induction of universal facts in Harvey's method, in which the beginnings of mathematical rationalization to measure reality are evident. Harvey is not the first to apply measurements and calculations to biological investigations—fourteen years before the writing of *De motu cordis* the *Ars de statica medicina* of Santorio had already been published—but Harvey is the first to succeed gloriously by this procedure. In Harvey's very simple calculations the expression of reality in terms of formal mathematical laws is still minimal. In Chapter IX of *De motu cordis* the author deals with the weight of the blood and the number of pulsations to perform a simple multiplication followed by a final computation between two figures. Measurements and calculations are to Harvey auxiliary methods

[11] *Exercitatio anatomica altera ad Riolanum,* p. 122.

in the task of clarifying the hydraulic mechanism of the animal. Harvey is still very far from thinking, like Galilei and Descartes (and later on to a certain extent Claude Bernard), that the book of nature is written in mathematical language, and still farther from believing that only that which is measurable is true.

Harvey's procedures constitute the first step in the adventure of measurement. In his intuitive physiology, of an anatomical figure moving in space, measurement is just a procedure. The present physicist and physiologist do not measure "realities" but "variations." Harvey nevertheless took the first step towards the present books of "mathematics for the biologists."

Seen from within, the scientific contribution of Harvey reveals to us the Janus-like condition of its author. One of his faces is ancient, the other modern. The former is less visible and yet forceful enough. The expression *"natura perfecta et divina nihil facit frustra,"* so full of ancient flavor, is used by Harvey with the gravity of one advancing a scientific argument. He believes that the motions of nature are brought about by a final cause accessible to the mind of the student and he plans to investigate it: *"sed de his convenientibus, cum de huiusmodi motus causa finali speculabimur"* (*De motu cordis,* VIII), he says, speaking of the circulatory motions of the blood. Still more, as a Christian, Harvey christianizes his ancient idea of nature, and aspires to discover in nature the image of God that created it: "It occurred to me that by these means [the observation of animals] we might discover not only many a great mystery of Nature, but also the image of the very Supreme Creator (*De generatione animalium,* Dedicatio).[12]

On the visible side of the medal is Harvey's strictly modern face. All the motivations of the mentality we call "modern" are present, some vaguely, some very clearly, in the work of Harvey. With this work, after the first attempts in Italy immediately preceding *De motu cordis,* begins modern physiology. *"Incipit physiologia nova."* In the determination with which the physiologists who came after Harvey devoted themselves to this physiology, to what was "new" in it, rests their claim to glory; in the belief that this "new" physiology is the only true and valid one, rests their secret weakness. Because, no matter how great it has been or it will be, the *physiologia nova* is not and cannot be, by itself, the *physiologia tota.*

12 Here is a curious similarity between the thought of Harvey and Fray Luis de Granada. In both—though more vividly and with greater ingenuity in Fray Luis—survives the idea of the world as a reflection of God, its one primary cause.

Harvey's Use of Galen's Findings in His Discovery of the Circulation of the Blood

FREDERICK G. KILGOUR*

VARIOUS historians of the discovery of the circulation of the blood have held conflicting views of Galen's effect and importance in the story. Some have held that Galen's errors obstructed the discovery; others, such as Joseph Payne[1] and more recently Donald Fleming,[2] have contended that Galen made valuable contributions. This paper shows the degree to which Harvey used Galen's work to support his own arguments and the extent to which he disagreed with Galen.

In Harvey's works dealing with the heart and the circulation of the blood,[3] Galen's name or the phrase "Prince of Physicians" occurs 56 times, of which five instances are in what amounts to quotations from Jean Riolan. In other words, Harvey uses Galen's name 51 times. Each reference does not, of course, imply a separate discussion; Harvey often uses Galen's name several times in a consideration of but one of Galen's experiments or theories. Moreover, in his *Prelectiones* Harvey often does not indicate his attitude toward a reference to Galen, which is not surprising in lecture notes. Among the four works examined there is some repetition. For instance, he refers to Galen's experiment designed to determine whether the pulse is a function of the arterial wall or the blood within the artery in his *Prelectiones, De motu cordis,* and his *Second disquisition to John Riolan.*

It should be pointed out that Harvey sometimes refers to Galen's work without mentioning Galen's name. In the celebrated Chapter IX of the *De motu cordis* which contains the only quantitative argument in which Harvey used actual measurements, his main object is to demonstrate that more blood is "pumped into the

* Yale Medical Library, Yale University.

1 Payne, Joseph Frank. *Harvey and Galen.* London, Henry Frowde, 1897.

2 Fleming, Donald. William Harvey and the pulmonary circulation. *Isis,* 1955, *46,* 319-27.

3 Harvey, William. *Prelectiones anatomiae universalis.* London, J. & A. Churchill, 1886, s. 73 recto-s. 80 verso. *Exercitatio anatomica de motu cordis et sanguinis in animalibus. With an English translation and annotations by Chauncey D. Leake.* Springfield, Illinois, C. C Thomas, 1928. "The first anatomical disquisition to John Riolan" and "A second disquisition to John Riolan; in which many objections to the circulation of the blood are refuted." In *The works of William Harvey, M.D.* London, Printed for the Sydenham Society, 1847, pp. 87-141.

arteries and the body than could be furnished by the food consumed." The concept that the blood was formed from ingested food was supported by Galen, yet the only reference to Galen in Chapter IX is to the experiment wherein Galen severed a small artery and showed that the blood drained from both veins and arteries. In short, the method employed in this paper of counting Harvey's references to Galen and segregating them into one group which Harvey used in his own support and another group which he rejected does not yield an absolute measure. It does show, however, that Harvey found Galen's results useful to him in developing his concept of the circulation.

Harvey specifically refers to or discusses Galen's findings and views on 33 occasions. Of the 33 references, Harvey employs 20 to support his own reasoning and 13 times he disagrees with Galen's findings or views. In each group there are, of course, repetitions as already pointed out.

Harvey most often calls on Galen for support when referring to his experimental observations, with one notable exception.[4] Examples of these results are Galen's experiment to show that there is only blood and not air in arteries, and his demonstration of an arterio-venous connection already referred to. In opposing Galen, Harvey often rejects his interpretations. In one reference, he accepts the observation but uses it to negate Galen's views on the motion of the blood: "The great man often agrees in this connection that *'all arteries arise from the aorta, and this from the heart, all normally containing and carrying blood.'* He says further, *'The three semilunar valves, placed at the opening of the aorta, prevent the reflux of blood into the heart. Nature would never have connected them with such an important organ unless for some great purpose.* If the 'Prince of Physicians' admits all this, as quoted in his very words from the book cited, I do not see how he can deny that the aorta is the very vessel to carry the blood, properly perfected, from the heart to the whole body."[5] Incidentally, as the last part of this quotation shows, Harvey often refers to Galen as though he were a contemporary. In a sense he was, for as an experimental biologist Galen had advanced to positions from which Harvey was the first to push on further.

In general it can be said that Harvey found some of Galen's observations and experimental techniques useful. At the same time

[4] Kilgour, Frederick G. William Harvey's use of the quantitative method. *Yale J. Biol. Med.*, 1954, *26*, 412.

[5] Harvey. *Exercitatio* (note 3), p. 51.

he rejected some Galenical concepts. Nor was Harvey always successful in replacing the older concepts with new. For instance, he rejected the Galenic idea that the blood served as the nutrient for the tissues,[6] which was not original with Galen. Harvey, however, was unable to evolve a concept of the function of the blood and the circulation, and it is doubtful that a fruitful idea could have been developed in the first half of the seventeenth century. "Yet," as John C. Curtis put it, "the phenomena of the very circulation used were so striking as to cry aloud for elucidation; for Harvey's own clinching statement that the heart drives into the aorta at least one thousand drachms of blood in half an hour, this *reductio ad absurdum,* which cut the ground from under the feet of his opponents, left him helpless in his turn to account for the need of so huge a flooding of the arteries."[7]

In summary, it would appear that Harvey found Galen's observations useful, but that many Galenic concepts were no longer productive in the sense of suggesting new areas for investigation. Harvey's most important contribution was his discovery of the circulation; in making this discovery, he used some of Galen's experimental studies on the vascular system. This type of event is typical of the evolution of science, and it can be concluded that Galen's work facilitated rather than obstructed the subsequent advances in the history of the knowledge of the vascular system.

[6] *Ibid.*, p. 73.

[7] Curtis, John C. *Harvey's views on the use of the circulation of the blood.* New York, Columbia University Press, 1915, p. 41.

Thoughts on Bibliographies and Harvey's Writings

E. B. KRUMBHAAR*

WILLIAM HARVEY worked and lived in the infancy of organized medical bibliography. Conrad Gesner (mid-sixteenth century) stands out as a conspicuous exception. Bibliography, and especially when on scientific subjects, was barely established as a field of literary endeavor by the sixteenth century. Even the name in its present meaning is surprisingly modern; long used to signify writing books, it shifted to writing about books, in France in the eighteenth century, and in England, following men like T. F. Dibdin, in the early nineteenth century. From Miss Brodman's *The Development of Medical Bibliography* we learn that periodical scientific publications practically began with the *Journal des Sçavans* (1665) and the *Philosophical Transactions of the Royal Society* (1665), i.e., both of them after Harvey's death. With them and others like them, medical publications changed from costly monographs appearing in small editions to shorter forms with a wider dissemination. Case histories, to be sure, could be found in occasionally published *consilia, epistolae, adversaria, opuscula, libelli,* and *fasciculi,* and these sometimes gathered in larger works such as Bonet's *Sepulchretum* and Morgagni's *De sedibus.* Incidentally, it might be noted that general bibliographies, never even hoping for completeness, reached their height in Robert Watt's *Bibliotheca Britannica* (1824) and Lowndes' *Manual* (1834), but thereafter were supplanted by special bibliographies of ever-narrowing fields and even a bibliography of bibliographies! The three bibliographies noted by Miss Brodman as advancing medical science in the seventeenth century were those of van der Linden, Lipenius, and Beughem. The most comprehensive of all medical bibliographies is the *Index-catalogue of the Surgeon-General's Library* in 58 volumes, now unfortunately defunct. This series, which Welch spoke of as perhaps the greatest of America's contributions to medicine, with the *Quarterly Cumulative Index Medicus,* will long continue as useful tools for scholars.

When invited by Dr. Fulton, to my very considerable surprise, to write on Harveian bibliography for this special number, inquiry

* Philadelphia, Pennsylvania.

elicited that the invitation was due to an article, completely forgotten by me, about a Harvey exhibit that I had written over twenty-five years ago and in which I had loosely used "bibliography" in the title.* Now Dr. Fulton is, of course, a noted contributor to our literature of a number of bibliographical studies himself, and I felt that this was an honor not to be refused. Also I have long since found it especially hard to turn down any invitation from that source. Since then I have learned a little about the subject. My first step toward orientation took me to the *Encyclopaedia Britannica* (1911 edition), which contains a scholarly article on Harvey by P. H. Pye-Smith, the Harveian Orator in 1893. It gives, *inter alia,* what "is believed to be a complete list of all the known writings of Harvey, published and unpublished." It was indeed surprising to learn that the "complete list" consisted of: (i) the *De motu cordis;* (ii) the two letters to Riolan (*De circulatione sanguinis),* published twenty-one years later; (iii) the *De generatione animalium;* (iv) letters to Caspar Hofmann, Paul Marquard Slegel of Hamburg, Giovanni Nardi of Florence, Dr. Horst of Darmstadt, and Dr. Vlackfeld of Haarlem, one on official business to Lord Dorchester, and one about a patient. The items in this "complete list" were all published in various ways after Harvey's death, several in the nineteenth century. The manuscript notes of his Lumleian Lectures (1616), *Prelectiones anatomiae universalis,* which are in the British Museum, were reproduced by the Royal College of Physicians of London in 1886, while a second manuscript, *De musculis, de motu locali &c,* was published in part by Dr. G. E. Paget (*Notice of an unpublished manuscript of Harvey,* London, 1850; see Keynes, 2d ed., No. 50). Pye-Smith also notes other works that Harvey was said to have written, but which were thought to have been lost either when his house was pillaged or in the Fire of London: namely, *A Treatise on Respiration; De usu lienis; Tractatus physiologicus; Anatomia medicalis* ("apparently notes of morbid anatomy"); *De generatione insectorum; De motu locali.* Excerpts from the last named, however, are contained in Paget's "Unpublished manuscript." In addition, in Harvey's second letter to Riolan, the *Observationes medicales* (also referred to as *Anatomia medicalis*) are mentioned merely as observations that he proposes to publish, and Paget's assumption that this was already in manuscript is questionable.

* Krumbhaar, E. B., Bibliographical matters pertaining to the discovery of the circulation of the blood, based on the tercentenary exhibit of Harveiana by the College of Physicians of Philadelphia. *Ann. med. Hist.,* 1929, *1,* 57-66.

Thoughts about the bibliographical aspects of Harvey's writings then turned, naturally, to the excellent work by Sir Geoffrey Keynes, which should remain the master word on the subject far into the future. First published in 1928, celebrating the three hundredth anniversary of the publication of Harvey's greatest discovery recorded in his *De motu cordis,* it had the unusual distinction in this field of requiring a second edition in 1953. It would be presumptuous here to dwell on the erudition, thoroughness, clarity of exposition, and command of the written word that these two books exhibit, especially in their introductory sections. Even in the necessarily dry details of collating the individual items one not infrequently meets enlivening touches. Of the 69 pages in the 2d edition devoted to the works, the *De motu cordis* requires 35 pages; *De circulatione,* 7; *De generatione,* 16; the *Opera omnia,* 6; and a miscellaneous group (letters, the account of "Old Parr," etc.), 5.

As Keynes indicates, he has restricted his presentation to published works by Harvey. He has purposely avoided the much larger field—a bibliography of lost or doubtful works by him; of material written in favor of or against Harvey's views; of developments occurring since Harvey's time; of studies about Harvey himself; of his impact on the progress of medicine in succeeding centuries—all proper material for bibliographical treatment. Let us hope that he, or one of his disciples, may restudy the material on one or more of these aspects. The recent "A Select Bibliography of Writings on William Harvey" in numbers 9 and 10 of *Current Work in the History of Medicine* published by the Wellcome Historical Medical Library (1956) provides a useful guide to what has already been done.

The *De motu cordis,* as we know, was first received with scepticism; but in a surprisingly short time a shift occurred to a wide acceptance of the circulation doctrine. Plempius, for one, first combatted Descartes' support of Harvey's views but shortly moved over to the other camp. A good picture of the change in Willis' *Works of William Harvey, M.D.* (pp. xlii-lii) offers a start for the scholar attempting study of this phase: Roger Drake, Regius, Rolfink, Ent, de Back, Riverius, Slegel, Trullius, Pecquet, Bartholin versus Primrose, Parisanus, C. Hofmann, Vesling, Plempius, Riolan, Folli, Gassendi. Here Conring, an influential physiologist of that period, should be added to Harvey's supporters.

It should not be forgotten that Harvey's great achievement, recorded in *De motu cordis,* was chiefly an arrangement and a demonstration rather than a new discovery; this is supported by the fact that most of the phenomena on which the treatise was based had already been described as isolated observations without leading toward establishment of the general concept of the blood moving continuously in a circle. Harvey's discovery of course could not be completely demonstrated before capillaries had been found (1660) by Malpighi, "greatest of microscopists," and for this, also of course, a microscope was a necessity. Malpighi, incidentally, was born in 1628, the same year as *De motu cordis* was published; and his discovery was made three years after Harvey's death. As E. H. Starling phrased it in a Harveian Oration, greater than Harvey's discoveries was his method of procedure in formulating and working out problems. Almost everyone knows of the long interval (the "nine years and more" in his dedicatory letter to Dr. Argent) between Harvey's demonstrated grasp of the principle of the circulation and his public statement of it in 1628. His first writing on the subject is to be found in his notes for his first Lumleian Lectures (1616), mentioned above. The Lumleian Lecturer in those days held a life appointment, giving annually a series of lectures covering the whole body. Harvey held the position for forty years. His notes on the heart, though necessarily brief, were sufficient to show that he was already on the right track, even if he did not give as detailed an exposition as Willis maintained.

De generatione animalium, Harvey's other great work, published in his seventy-third year, treated a subject so complex and abstruse that in the then state of physiological knowledge but little might be expected of it. Yet all admit its merit, as well as its frequent errors, and it has even been claimed to be as important for its field as *De motu cordis* is for the circulation. It includes at least two points of first importance: (i) that all animal life, even that of man, came from the egg (a seventeenth century *omne vivum ex ovo);* and (ii) that from the egg the embryo developed by the process of epigenesis, successive differentiation from a relatively homogeneous rudiment. Garrison and Morton add a third important item, namely, that Harvey's chapter on labor "represents the first original work on obstetrics to be published by an English author." His observations were not made with the naked eye, as is generally assumed. Though he never used the microscope, which even in its compound form had been invented before the seven-

teenth century, his known use of lenses (*perspicilla*) must have materially aided his observation of the early vascular development in the chick.

The *De generatione,* like *De motu cordis,* was many years in preparation. Harvey's friend, Dr. Ent, who was responsible for getting the book into print, speaks in his dedicatory epistle of "the treasure [the finished manuscript] so long concealed" and of Harvey's notes on the generation of insects that had been lost in the turmoil of the Civil War.

Did Harvey reason by induction, deduction, or a mixture of both? In the Introduction to *De generatione* Harvey discloses his views on "The manner and order of acquiring knowledge": universals being more generally known to us, science proceeds by reasoning from universals to particulars, yet the comprehension of universals is based upon the perception of individual things by the senses, i.e., by induction (this passage is slightly paraphrased from Willis' translation of 1847). Harvey was a friend of Francis Bacon, whose *Novum organum* appeared in 1620, and though his own ideas had been formulated before this date, he may well have acquired the habit of thought from Bacon. Yet Harvey is also said to have scoffed at him that "he writes philosophy like a chancellor." Harvey was a painstaking experimenter and observer, making at least some original observations and deducing his conclusions one by one; as Huxley put it: "Harvey's method is that of Galen, Realdus [Colombo], Galileo and of every genuine worker in science." If his deduced conclusions taken together led to a generalization such as the circulation, this would clearly seem like an induction, but such distinctions had better be left to philosophy for elucidation.

Harvey's brief report on the anatomical examination of the body of Thomas Parr, aged one hundred and fifty-two years, is interesting to a pathologist at least, both for the picture presented and as an example of the way necropsies were conducted in the mid-seventeenth century. The clinical picture, the post-mortem changes, and ideas on the cause of death are intermingled in a way that after all is logical, considering that long before the days of professional pathology, or any other specialty, emphasis was so predominantly on presenting a clear, coherent story. An item about Parr's memory is worth noting: contrary to the usual condition in the aged, his memory for recent events was much better than for those of his youth. This account first appeared in John

Betts' *De ortu et natura sanguinis* (1669) and in abstract in the *Philosophical Transactions* of the Royal Society in the same year. According to Keynes, it is the only one of Harvey's miscellaneous writings printed in the seventeenth century. Another interesting pathological observation by Harvey is a case of rupture of the heart described in his second letter to Riolan (see p. 127 of R. Willis' *Works*). This is not only an early, perhaps the first, recorded instance of cardiac rupture, but it also includes the remarkable statement that it had "apparently been caused by an impediment to the passage of the blood into the arteries."

I have not considered *De amore, libidine et coitu animalium,* mentioned by G. E. Paget in his Harveian Oration (1866) as having probably been in existence in 1650.

It is to be hoped that perhaps these topics, like the supposedly lost treatises that Pye-Smith enumerates and others that I have touched upon, may be more fully discussed in other articles of this special number.

William Harvey, Weir Mitchell, and the College of Physicians of Philadelphia

W. B. McDANIEL, 2d*

Generalities

THE College's continuing affair with William Harvey had a modest but not uninteresting beginning in the winter of 1788/89, when its library was started with two donations of books by Dr. John Morgan. Among the books was a copy of the 1766 London edition of the *Opera omnia* which had been sent from England to Dr. Morgan in 1769 by the Bishop of Worcester. Until 1846 at least, this was the sole Harvey edition in the library.

With the founding in 1864 of its Samuel Lewis library (now not maintained as a separate collection), the College savored its first heady draught of the bibliophilic wine. It had also acquired as a Fellow, in 1856, a 27-year-old physician named Silas Weir Mitchell, who was to play an increasingly magistral rôle in its affairs for the next 58 years. At his death in 1914, and largely as a result of his private passion (publicly exposed), none among the many distinguished physicians of the past whose lives and works could be studied in the College library in uncommon depth was to be found more fully fleshed than William Harvey. Most generally known of Weir Mitchell's contributions to Harveiana are his privately printed pamphlets "Some memoranda in regard to William Harvey, M.D." (1907); and "Some recently discovered letters of William Harvey, with other miscellanea" (1912),[1] the latter containing a bibliography of Harvey's works compiled by the Librarian of the College, Charles Perry Fisher.

In observance of the tercentenary (1928) of the publication of *De motu cordis,* the College, though Mitchell was then long dead, had R. Lier & Co. of Florence print for it 250 copies of a facsimile

* College of Physicians of Philadelphia.

1 Though the words *Transactions of the College of Physicians of Philadelphia* appear above the title of this publication, and the printing was done by the College's printer, the material did not derive from a numbered volume of the *Transactions* but was published separately, in a different format.

These are Harvey's letters to Lord Feilding (Keynes, 2d ed., nos. 53 and 54). A handsomely bound volume in the College labeled "Letters and notes relating to William Harvey," bequeathed by Weir Mitchell, contains letters exchanged between Mitchell and Lady Denbigh that indicate that the College would have obtained possession of the letters, had Sir Thomas Barlow, President of the Royal College of Physicians, not accepted the opportunity to match our College's offer for them.

of the little book, the Royal College of Physicians, London, commissioning a like number. It also in the same year held a large exhibit of the College's Harveiana. A detailed, illustrated report on the exhibit by E. B. Krumbhaar[2] (later President of the College) movingly demonstrates the power of Mitchell's enthusiasm as it had spread from himself to others. By the same token, it reveals that the affair had reached that difficult stage at which the partners must exercise considerable ingenuity to surprise each other. Nearly three decades after the publication of Krumbhaar's comprehensive report, comparable ingenuity must be sought to supplement, rather than plagiarize, it.

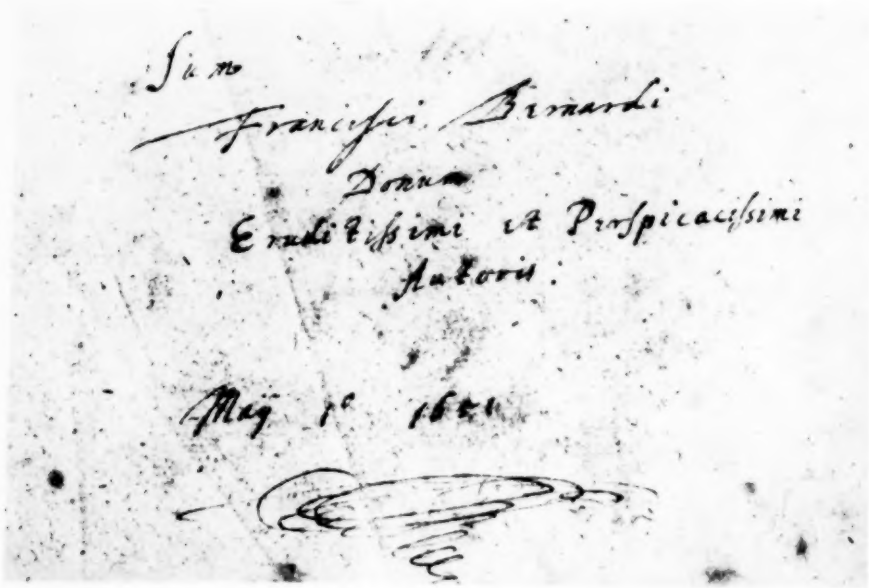
SOME COMPARATIVE HOLDINGS, 1912:1957

It may be recalled that when Keynes first published his Harvey bibliography in 1928, he referred to Fisher's bibliography of 1912 as "the only important source of bibliographical information hitherto available." Of the 46 editions he listed, Fisher reported that the College and the Library of the Surgeon-General's Office (now our National Library of Medicine) each had 33; the British Museum, 27; the New York Academy of Medicine, 21; the Boston Medical Library, 19; and the Paris Faculté de Médecine, 16.

With another Harvey tercentenary reminding us anew of our common debt, it seemed forgivable to measure again the relative Harveian ardor of these libraries, using the second edition (1953) of Keynes' bibliography as the point of departure. The measurement can be regarded as only relatively valid, unfortunately, since Keynes in preparing this edition did not probe all of his earlier correspondent libraries for acquisitions made since the publication of his first edition.[3] Our measurement took account of the seventeenth and eighteenth century editions only, and excluded further the two so widely held that Keynes resorted to use of an "etc." Quite without sportsmanship, we included College holdings not registered by Keynes. Of the 36 editions checked, at least 11 appear to be held by the Faculté de Médecine, 17 by the Boston Medical Library, 22 each by the National Library of Medicine and the New York Academy of Medicine, 23 by the British Museum, while 30

2 Krumbhaar, E. B. Bibliographical matters pertaining to the discovery of the circulation of the blood, based on the tercentenary exhibit of Harveiana by the College of Physicians of Philadelphia. *Ann. med. Hist.*, 1929, *1*, 57-86.

3 For this reason, or perhaps in some cases through a clerical error, the College, for instance, is not registered by Keynes as holding, among the 17th-18th century editions, his nos. 8, 32, 35, 36, 41, 45, 49; while it is incorrectly listed as holding a copy of no. 18.


Sum
Francisci Bernardi
Donum
Eruditissimi et Perspicacissimi
Autoris.
Maij 1° 1621

can be found in the College. But this graph could be even more outrageously misleading, of course, if contiguous reference were not made to entries into the joust, in the decades since 1912, of a number of new or resurgent competitors more generously endowed with the ardor of youth and a few loose guineas. At Uppsala, for instance, the Bibliotheca Walleriana offers to the manes of Harvey 31 of these editions, the Wellcome Historical Medical Library, London, 27, the Historical Library of the Yale Medical Library, 20, the Logan Clendening Library of the University of Kansas, "all the various editions of *De Motu Cordis,* with two exceptions, and all the editions of his *De Generatione Animalium.*"[4] All that we wish to suggest, really, is that the College remains a comparatively well-equipped laboratory for students of the great physiologist.

SUM FRANCISCI BERNARDI
DONUM ERUDITISSIMI ET PERSPICACISSIMI AUTORIS
MAIJ 1° 1651

This inscription (I am the property of Francis Bernard, a gift of the most learned and keen-sighted author) appears on the verso of the first leaf (a blank) of one of the College's three copies of the first edition (London, 1651) of *De generatione.* Pasted in the copy is an undated note, in Weir Mitchell's hand, that reads as follows: "This most precious book must have been once in the hands of William Harvey. It was bound by Emily Tuckerman. The device is that set up in Padua by Harvey. This book I leave to my dear son—John." (It was presented to the College by Dr. John K. Mitchell late in the year of his father's death.) Assuming the facts to be as stated in the inscription, one must concur, and with like emotion, in Mitchell's belief that Harvey must once have held this book in his hands.

According to Keynes (*Bibliography,* 2d ed., p.vi), "It is to be noticed that Harvey's aloof character is shown by the absence of any record of a presentation copy of *De Motu Cordis,* or of any other of his books." It may be helpful, therefore, to extend in some measure brief notices of this copy already snugly tucked away in the literature. Mitchell himself (in his *Memoranda,* p. 23) merely quoted the inscription (in part, inaccurately), regretting that it was not in Harvey's hand, and also Munk's *Roll* on Bernard.

[4] R. A. Major, in the admirable *Books and libraries at the University of Kansas,* 1955, *1,* 1-3.

Norman Moore, quoting this notice in part, corrected the date Mitchell assigned to the book but naturally not the Latin of the inscription.[5] J. L. Thornton has referred to the existence of the book, but without locating it or citing either Mitchell or Moore.[6]

The Francis Bernard whom the book claims as its owner was, there seems no reason to doubt, and as Mitchell himself indicated, the learned English physician of that name (1627-1698) who served as physician to James II and to St. Bartholomew's Hospital, but who is perhaps better known today as one of the greatest book collectors of his time. The circumstances that led the aged, infirm, and "aloof" Harvey to present a copy of his book, fresh from the press, to this 24-year-old young man, about whose early career nothing seems to be known, remain a mystery of superior proportions. The famous sale catalogue (1698) of Bernard's books (which, incidentally, dates one *De motu cordis* 1627, and another, containing the refutations of Parisanus and Primrose, 1634!) indicates that Bernard had two of the three 1651 Amsterdam editions of *De generatione* but does not list the London quarto of the same year. The presentation copy evidently encountered an anterior, more private fate. In this connection, one recalls a passage in the sale catalogue's introductory words "To the Reader" that explains the nonappearance of many of Bernard's valuable books in the catalogue as stemming from the owner's earlier "easiness" of temper in lending his books, and from "an infelicity of a worse Nature" which in the seventeenth century bibliopole's lexicon might be defined, one suspects, as the reprehensible habit of presenting some of one's choicest books to one's friends.

Returning to the other end of the axis: How did this precious book come into Weir Mitchell's possession? Inserted between the blank leaf containing Bernard's inscription and the title (in this copy the frontispiece is bound behind the title), are two leaves of heavy paper, each containing, on the recto, two affixed bits of writing on different papers. The lower one on the second leaf reads, in Mitchell's hand: "Dep$^{d.}$ Weir Mitchell [.] Presentation copy of De Generatione animalium"; then, in large letters: "Wm. Harvey to Francis Bernard." The upper one on the same leaf, not in Mitchell's hand, reads: "Obtained from Phila Library Company in Exchange. S. Weir Mitchell."

[5] Moore, N. *History of St. Bartholomew's Hospital.* London, Pearson, 1918. Vol. 2, p. 513.

[6] Thornton, J. L. Francis Bernard. *St Bart's Hosp. J.*, 1946, *50*, 100.

The Library Company of Philadelphia was founded in 1731 by Benjamin Franklin; Weir Mitchell was one of its directors from 1875 until his death. To it was annexed, in 1792, the "Loganian Library," the library of James Logan, confidential adviser to William Penn, which, in turn, had been enriched in 1776 by 1,300 volumes that had been collected by Logan's son William and by the latter's namesake uncle, Dr. William Logan (1686-1757), of Bristol, England. In the Loganian Library at the time of its annexation to the Library Company, according to the printed Loganian catalogue of 1837, was a copy of the London 1651 *De generatione*. As the Library Company is still in possession of that copy (which bears the inscription "J. Logan"), it is clear that the Bernard copy was a duplicate, received from a source now unknown, and the perhaps unique value of which even the astute Mitchell was not in a position to appraise fully. One is tempted to wonder which of the Library Company's books might yield the reciprocal statement, "Obtained from Dr. Mitchell in Exchange."

There remains in the book but one other tangible mark of pauses by the wayside in its passage from the hands of Francis Bernard to those of Weir Mitchell. To the upper half of the first inserted leaf is affixed a personal signature cut from paper comparable with that on which the book is printed. Missing from this copy (though it is present in the College's other two copies) is C4, a blank. Clearly, the blank was discarded in binding, for one reason or another, but the signature appearing on it was carefully preserved in this way. When the book was catalogued by the College, the signature was deciphered, it would seem correctly, to read: Tho: Hodson.

There was a Thomas Hodson resident in Philadelphia within a decade of the colony's founding. That he was a man of such substance as to make him a credible owner of the book is suggested by the fact that, in "William Penn's Account with Samuel Jennings, Rec'r Gen, 1690-93,"[7] there is the record of a payment "To Willm. Biles for work done by Tho. Hodson's servants." Hodson would, therefore, have come to this country several years before the death of Francis Bernard. Two Thomas Hodsons present themselves as inconclusively in the next century or so. One was married in Philadelphia in 1797 to a Rebecca Jones.[8] The other published in London in 1800 a work entitled *The accom-*

[7] *Penn. Mag. Hist. Biog.*, 1911, *35*, 202.
[8] *Publ. Genealog. Soc. Penn.*, 1926, *9*, 132.

plished tutor; or, complete system of liberal education; and, in 1805, with John Dougall, a *Cabinet of the arts, being a new and universal drawing book.* It can hardly be doubted that Weir Mitchell would seek to identify further this predecessor in ownership of the book, as he did in the case of Bernard. His failure even to allude to the signature was perhaps a tacit, as ours is now an overt, admission of defeat. The bold chirography raises, to us, the specter of a Philadelphian of the late seventeenth-early eighteenth centuries.

These unresolved matters do not quite conclude the interesting features of this copy. For on the verso of the frontispiece—in a hand close to that we take to be Bernard's and certainly not that of Hodson—we find some Greek and Latin quotations, correctly documented in the margin and culminating in the declaration: *Harvaeus medicis praestantior omnibus unus.*[9] The first, in Greek, is attributed to the *Iliad.* The second, in Greek with a Latin version under it, is referred to Cicero's *Tusculan disputations,* and is perhaps annotated by a quotation from Pliny's *Letters* to the right of it.

Knowledge of the presence of these inscriptions is needed to grasp the meaning of the otherwise puzzling fourth bit of writing affixed to the preceding inserted leaves. Mitchell had evidently observed the inscriptions and had them translated by a classicist friend, then affixed the friend's scribble without indicating its relationship to the inscriptions.

The line from the *Iliad* is translated as "One medical man is equivalent to many others" (the translator captiously adding the Homeric qualification, "in cutting out arrows and applying alleviating medicines"). As to the second line in Greek, which has Cicero's Latin version under it, he says: "The other Greek words—'This is Demosthenes'—do not seem to have any relation to the Greek above—except as alluding to his great superiority over other orators." Finally, he translates the declaration on Harvey as, "Harvey is superior to all physicians." Left untranslated is the quotation from Pliny. The words convey Pliny's delight at hearing his name coupled with that of the great historian Tacitus.

The sequence of thoughts as we read down, then, seems to be: The skill of the physician equates with the combined skills of many other men; Here is our Demosthenes—Harvey, who excels all other

[9] *vivis*—which would be a more satisfactory reading than *unus*—unfortunately seems unjustifiable.

physicians. Pliny was perhaps enlisted to support the comparison of the two supreme figures. The same hand seems to be responsible for corrections on two pages (13 and 24) that are called for in the list of *emendata* on the final page of the book. All of this suggests the hand, mind, and heart of the book's first owner, the learned and easy-tempered Dr. Francis Bernard.

Reminiscent though it be of a celebrated American university classic of rhetorical anticlimax*: Vivant Harvey, Bernard, Mitchell!

[* The author obviously did not attend Yale University. *Ed.*]

A Latin Translation of Ibn Nafis (1547) Related to the Problem of the Circulation of the Blood

CHARLES D. O'MALLEY*

ALTHOUGH it was long believed that the discovery of the circulation of the blood had its inception in Europe in the middle decades of the sixteenth century, about a quarter century ago it was learned that an earlier description of the pulmonary transit of the blood had been written in the thirteenth century by a Moslem physician, Ibn Nafis. The discovery either in the orient or the occident presumes a prior dissatisfaction with the reigning Galenic doctrine, and it seems likely that the first attack upon this well-established doctrine would have been an oral one. Thereafter, at least in the case of Europe, there was published, as we know, a certain amount of pioneer research into isolated factors which was ultimately brought together to form the description of the new doctrine of the lesser circulation or pulmonary transit of the blood. While it is unlikely that we shall ever know the oral doubts expressed in Europe, we can, however, trace a certain amount of the preliminary work published, some of which must certainly have implied doubt, that of Berengario da Carpi, Niccolò Massa, Andreas Vesalius, Giambattista Canano, and oddly enough, even of that arch-Galenist Jacobus Sylvius, leading to the relatively complete descriptions of Servetus, Valverde, and Colombo.

There has long been some question as to whether or not the prior work of Ibn Nafis had become known in Europe and contributed to the discovery there. Thus far, however, no relationship has been found between his Commentary on Avicenna, which contains the description of the lesser circulation, and western medical thought in the sixteenth century. Nevertheless, in a work entitled "Sixth consideration on the pulse taken from the Arabic book of Siraxus," published in Venice in 1547, are imbedded some interesting and suggestive, but not conclusive, statements drawn from an unidentified work of Ibn Nafis translated by Andrea

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Alpago.* These statements represent problems which oppose the Galenic doctrine, as it was related to the heart and arterial system, and orthodox answers which seek to reconcile such doubts or contradictions. It appears as if the problems and their answers were a kind of academic exercise allowing Ibn Nafis to express doubts which he held and yet by his answers, unsatisfactory as they might be, to remain within the Galenic camp. Hence it is suggested that these statements are drawn from an early work prior to his final description of the pulmonary transit of the blood and therefore his open break with Galen. Indeed, even in this treatise Ibn Nafis is referred to as speaking on occasion "in disproof of the opinion of Galen."

Five problems of some importance and their answers, drawn from this treatise, are presently considered. The first of these problems recognizes the Galenic doctrine as declaring that upon dilatation of the heart cold air is drawn to it from the lung, and upon constriction of the heart, heated air and superfluities from the spirits in the arteries are expelled from the heart to the lung. Yet, if upon dilatation the heart is filled with cold air, there is no space remaining for the superfluities drawn off originally from the arteries. The answer presented to this problem is merely that the dilatation of the heart provides more space than is needed for the air drawn from the lung and thus there is space for the superfluities from the arteries. The second problem is the reverse of this. If the dilatated heart attracts and is filled with the spirits from the arteries, then there is no space for the air from the lung. The answer is, of course, that the amount of spirits drawn from the arteries is not enough to fill the cavity of the heart and therefore there is space for air from the lung. It may be added that, purposely or not, the terms "superfluities of the spirits" and "spirits" are sometimes used interchangeably, and at times it is difficult to determine whether the author thought of the arteries as filled only with spirits or with spirituous blood. Again this may have been done purposely.

The third problem should have been more immediately destructive of the Galenic doctrine. According to that doctrine, when the lung is dilated to draw air, the heart is constricted, and when the lung is constricted to expel air, the heart is dilated, but

* "Consideratio sexta de pulsibus assumpta ex libro Siraxi arabico, & in latinum conversa per Andream Bellunensem physicum." In: *Avicenna philosophi praeclarissimi, ac medicorum principis libellus de removendis nocumentis, quae accidunt in regimine sanitatis.* Venice, 1546, ff. 24v-30r.

this, as the problem states, is impossible for several reasons: (i) "While the heart is moved five times the lung and chest are moved once," and "the motion of breathing can be retarded for a period of time during which the heart is moved twenty times"; (ii) when preparatory to holding one's breath a large quantity of air is inhaled it should be sufficient to fill the cavity of the heart, leaving no room for the spirits; (iii) so long as the breath is held there should be no further dilation of the heart to attract spirits from the arteries or constriction to expel spirits into the arteries. The unconvincing answer is that the heart has a "natural-voluntary" motion, that is, one controlled either consciously or unconsciously, such as the movements of the eyelids. In short, it is adaptable.

The fourth problem recognizes that the aorta has three valves at its origin, and that according to some these valves close this orifice of the heart at dilation, while according to others at constriction. In either case these valves would prevent movement of the spirits between arteries and heart. The answer is that there are spaces between the valves which the spirits, because of their subtlety, are able to penetrate.

The fifth problem, referring again to the valves, notes that if they prevent transmission of the spirits, then it may be hypothecated that a quantity of spirits is transmitted to the heart from the veins. But such a hypothesis must be false since the Galenic doctrine states that transmission of spirits to the heart is solely from the arteries. This, however, means that the quantity of spirits in the arteries is very great, and at the constriction of the arteries the quantity which goes to the heart must be sufficient to fill its cavity, since the expansion of the heart in the chest is limited by other organs and since only one ventricle of the heart receives air and spirits. Indeed, it seems likely that this ventricle could not contain the spirits, let alone the air from the lung. The answer given is that when the spirits have been distributed through the whole arterial system the superfluous quantity is not large, and the spirits which reach the arteries upon constriction of the heart are in proportion to the quantity necessary to fill their internal capacity. Furthermore, the aggregate filling the arteries is not merely spirits. Presumably the other material is blood, although how the spirits returning to the heart are freed from the blood is never explained.

Thus in these five problems we find expressed considerable doubt as to the orthodox Galenic doctrine in so far as it concerned the arterial system. The left ventricle of the heart is not large

enough to contain both the air and spirits that its supposed functions required, and the presumed necessary relationship between the motion of lung and heart can be interrupted or suspended without stopping the heart's function. The fourth and fifth problems had considerable potential implication. The closing off of the aorta by the valves at its origin, either at dilation or constriction, could allow a hypothesis that spirits are transmitted from the veins which in turn would imply transmission from artery to vein. Although immediately denied, yet such heterodox opinion did exist in theory, and elsewhere in the treatise Ibn Nafis does speak somewhat vaguely of pores of the arteries which could supply some kind of passage for egress of spirits into the extra-arterial regions of the body. It would perhaps be overbold to suggest that in this idea was the thought of some kind of connection between artery and vein. However, Ibn Nafis specifically remarks that "[only] one [ventricle] is the concavity of the spirits and of the air," and in both problems and answers it is recognized that this left ventricle is concerned only with air and spirits. There is no mention anywhere of blood permeating the cardiac septum from right ventricle to left.

Nowhere in the account is there any mention of the sources of the remarks of Ibn Nafis, and it may well be that other portions of the work, for example, on the heart and the venous system, would make his position much clearer. Furthermore, we are unaware whether this account is taken from an early or a late work. If we consider that the problems were presented by him as well as the answers or explanations, it could well be the case that his conservative explanations represent an early position, whereas the problems remained to cause him misgivings and ultimately had some share in the promotion of his description of the lesser circulation. These, however, are matters which can only be studied and determined by some one fully qualified to employ the Arabic texts.

If now we consider the Latin version as such and in relation to the sixteenth century, the first question that arises is why did Alpago choose to translate this particular work. There appears to be very little exact information about Andrea Alpago. We do not know exactly when he was born or when he died, although his life span may be said to cover the latter part of the fifteenth century and the early part of the sixteenth. His first published translation was that of Avicenna's *Canon*, Venice, 1527; since it contains a privilege against unauthorized printing of the work, dated 1523,

we may assume that publication was contemplated at least this early, and since there is a foreword by the translator it is possible that he was alive as late as 1527, although a second foreword by his nephew Paolo Alpago casts some doubt upon this. Furthermore, Andrea Alpago is mentioned as a physician of Belluno, and we may therefore assume that at one time he had studied medicine at some Italian university, possibly although not certainly at Padua. A second and revised edition of the *Canon* published by the nephew Paolo Alpago in 1544 included a Latin version of an Arabic life of Avicenna which had turned up among the papers of the deceased Andrea. Since the Latin version was the work of Niccolò Massa, it seems very likely that he was acquainted with Paolo Alpago, and he may well have been acquainted with uncle Andrea. Therefore, what he has to say about the letter seems likely to bear the stamp of authenticity.

> Andrea Alpago of Belluno, a man outstanding in our age for both character and learning, was so intensely concerned with the usefulness of knowledge for mankind that in the manner of the ancient philosophers, despising riches and his own advantage, throughout almost the whole of his life he chose to seek the truth of things for the benefit of mankind. In witness of this, when he had left his native land he travelled to Syria where he remained for more than thirty years studying diligently so that he might master the Arabic tongue; he became so proficient that he translated no few volumes of very learned Arabs into Latin as well as faithfully revising such translations as had been made earlier. . . . When formerly the eminently learned Paolo Alpago, nephew of Andrea, consigned [the revised text of the *Canon*] to Thomas Junta . . . he discovered among his uncle's possessions an Arabic text on one leaf of which there was written in Arabic a life of that famous man Avicenna by his disciple the Arab Sorsanus, and he expressed his regret that he had hitherto found no one who could translate this life of Avicenna. However, after I, who have always sought to guard carefully the fame of the most eminent men, had learned of Marcus Fadella of Damascus, interpreter for the Venetian merchants of Damascus, and had given him the task of rendering the words of Sorsanus into the vernacular, I decided to translate them into Latin.

Thirty years, or even half of that, would certainly have been sufficient time for Andrea Alpago to have gained a wide knowledge of the Arabic medical writers. Moreover, since his major translation was that of Avicenna's *Canon,* it may be assumed that he would have studied the various Arabic commentaries upon that work. This is re-enforced by the fact that in his foreword Andrea Alpago writes:

> After I, Andrea Alpago of Belluno, physician, had revised the books which Avicenna wrote on medicine . . . from the Arabic text, noticing that many excellent philosophers and physicians were greatly handicapped be-

cause of an unfamiliarity with the names of medicinal simples, as well as of other Arabic terms, and that varying and erroneous expositions were made upon the aforesaid books, I decided . . . to explain in alphabetical order and to the best of my ability all the Arabic words found in these . . . volumes according to Ebenbitar, Ebenefis [i.e., Ibn Nafis]. . . .

Moreover, Andrea appears to have had an interest in the history of Arabic medicine and Arabic writers on the subject, for he remarks that he had translated the great work on the lives of the physicians, *al-Kifti:* ". . . the Arabic account of the lives of Arab and Greek physicians translated by me from Arabic into Latin." For all these reasons there is likelihood that Alpago was acquainted with that commentary of Ibn Nafis in which he describes the lesser circulation, although possibly his Galenic background prevented him from realizing its significance. Nevertheless, he may have reflected a certain growing disquietude with the Galenic description of the movement of the blood which existed in the early sixteenth century, but as yet had not the temerity to present a refutation in the permanence of the printed word. All this, of course, is only hypothesis, and the very simple fact must not be overlooked that the treatise may have been translated merely as a treatise on the pulse. Possibly the answer lies in the various other unpublished manuscript translations which Paolo the nephew possessed and which may some day come to light.

Finally, turning to the nephew Paolo Alpago we must enquire why out of the various manuscripts which he inherited from his uncle did he choose to publish this particular one in 1547? Again an hypothesis may be ventured. By this final date there certainly was dissatisfaction with the Galenic view of the movement of the blood, and it is known that the question was being examined with greater attention. As yet no one had ventured to express himself in print as denying this long-established doctrine, although already some attention had been paid to the valves of the veins, and Massa and Vesalius had expressed some doubt in regard to the permeability of the septum of the heart. It is possible that such an atmosphere determined the choice of this translation for publication, although there is no sound evidence to support such a view. Moreover, there is no indication that it had any influence, although the possibility that it may have planted some seed which eventually developed into heterodox thoughts on the Galenic doctrine and thereby contributed in some measure eventually to a true understanding of the movement of the blood cannot be entirely ignored.

On the Death of Harvey

A premature threnody by Nikolaas van Assendelft

Edited by W. W. FRANCIS*

The original is in Dutch rhyming couplets, and its publication in 1650, seven years before the event, constitutes its chief interest. Signed N. v. A., it is prefixed to van Assendelft's translation of *De motu cordis, Vande Beweging van't Hert,* a rare little book with two title-pages, engraved and printed, dated respectively "1650" and "MDCL." The poem is headed in Dutch, "On the Death of the learned Master Gulielmus Harvejus, in his lifetime Professor (High-teach-master), and Physician of the King of England." If van A. survived a year, he must have been embarrassed by two editions of the living Harvey's *De generatione* appearing in Holland in 1651. He also translated the admirable *De corde* of his countryman, J. de Back, both versions of which are reprinted in volume 5, 1926, of *Opuscula selecta Neerlandicorum.* The untimely obituary has been Englished for me by Mr. John Van de Leuv, a senior medical student at McGill and officer of our Osler Society. Any mistakes are probably due to my metrical tinkering.

The English light that used to shine so bright
And unto Medicine a new glory gave,
Arousing wonder in the eyes of all,
Has its clear rays withdrawn, alas, too soon.
Closed is the mouth that ere it had played through
Its rôle, replete with golden scholarship,
Pregnant with wisdom, was to the perplexed
A lodestar on the yet untrodden paths.
Closed also are the hands, alas, too soon
Which often plunged the steel into the hearts
Of beasts and gathered knowledge from the brutes,
Searching their entrails, muscles, fibres, till
At last and of a certainty he saw
A sun break through the murky darkness, yes
A sun which drives away the fogs and fumes,
Illuminates our senses, tells us of
The pitfalls and diseases which afflict
Our feeble human race, a sun indeed
Which guides us through the deepest mysteries,

* Osler Library, Montreal, Canada.

Where lack of knowledge often made us fail.
This knowledge dropped like honey from his tongue
Whene'er he pleaded for the pure, clear truth,
Clothing his arguments with reason, and
Supporting them with many experiments.
Forgive us now our inexperience,
If we have not explained it all aright,
If our interpretation be at fault,
Making the sun a little star in Dutch.

Op de Doot vanden Geleerden Heer
GULIELMUS HARVEJUS,
in zijn leven Hoog-leer-meester,
ende Genees-heer des Konings
van Engelant.

Het Engels licht, dat eer zo heerlijk blonk,
En d'Artzeny een nieuwe glory schonk,
Verstrekkend' in een yders oog een wonder,
Trok, laas! te vroeg zijn klare stralen onder,
En sloot de mont, die, eer hy zijne Rol
Had uitgespeelt, van gulde Leering vol,
En zwanger van Geleertheit, den verlegen'
Een leitstar was, in ongebaande wegen.
Hy sloot zijn mont, en handen laas! te vroeg:
Warr mê hy't staal zo vaak in't Herte joeg
Der beesten, en zoog leering uit de Dieren,
Deursnuffelend' haar ingewant, en spieren,
En vezelen, tot dat hy eens, gewis,
Een Zonne zag in deze duisternis:
Een Zonne, die de nevelen en dampen
Verdrijft, ja licht de zinnen om de rampen
En qualen van de zwakke menschlijkheit
Te kennen: ja een Zonne, die ons leit
In't diepste van zo veel geheimenissen,
Waar in ons vaak d'onwetenheit dee missen.
Dit droop wel eer, als honig, van zijn tong
Wanneer hy voor de zuivre waarheit dong;
En pleitende zijn redenen beklede,
En stutte met zo veel Ervarentheden.
Vergeef het nu onz' Onervarentheit,
Indien w'et niet recht hebben uitgeleit,
En hebben wy't niet kunnen al ontwarre,
Zo wert die Zon in duitsch in kleine starre.

N. v. A.

Notes and Events

Edited by Dorothy M. Schullian, Ph.D.*

An unnoticed English version of Harvey's DE MOTU CORDIS *London, 1714*

There seems to be no published record of the fact that a slightly abridged version of the first English translation of Harvey's *De motu cordis* was published in London in 1714. Although the volume in which it appears must be considered something of a rarity, there is a copy in the Wellcome Library and another in the library of the Royal College of Physicians of London.[1] There, in the very temple of Harvey's fame, it seems to have escaped the keen eyes of a long succession of erudite physicians seeking any scrap of new information about Harvey or his writings.

The medical collections made and published by Daniel Leclerc and Jacques Manget towards the end of the seventeenth century are celebrated, but it is not generally known that a work in English and based on their *Bibliotheca anatomica* was published under the auspices of a group of London booksellers in the years 1711-14. The inclusion of the Latin text of the *De motu cordis* in the volumes of Leclerc and Manget has long been on record and is duly noted by Keynes (No. 44-45). The English work referred to above is actually entered in the published catalogue of the Royal College of Physicians (1911) under the names of Leclerc and Manget. Its full title is as follows:

> *Bibliotheca anatomica, medica, chirurgica,* &c. Containing a description of the several parts of the body; Each done by some One or more Eminent Physician or Chirurgeon; with their Diseases and Cures. Wherein are not only all the Tracts of Use that are in the Second Edition of the *Bibliotheca anatomica,* lately published by *Daniel Clericus* and *Jacob Mangetus,* in Two Volumes in Folio, but an addition also of near double the Number of other Curious Tracts, which were either omitted in the said *Bibliotheca,* or have been published since, some of them *translat'd,* others *faithfully abridg'd;* very few of which were ever before in *English.* Illustrated with several Hundred Figures, done by the best Artists, and from the truest Designs; as will appear from the Name to each Figure. Vol. I [-III]. In the Savoy: Printed by John Nutt; and Sold by W. Lewis . . . Dan. Brown . . . and J. Morphew near Stationers-Hall. 1711 [-1714] 4to.

An unusual feature of this work—and one which may account for its present rarity—is that it was published serially in monthly parts, a mode of publication then coming into vogue for more popular works but which

* Cleveland, Ohio.

1 There are also copies in the British Museum, National Library of Medicine (U.S.A.), and the Yale Historical Medical Library.

may have been inspired by such periodical publications as *The Works of the Learned* which offered its readers abstracts in English of important foreign books, many of them on medical and scientific topics. The first volume (we are told on the title-page) "Contains 14 Months, and finishes whatever relates to the Bones, with their Diseases and Cures, containing above 60 several Tracts, and many large Copper Plates, in which are above 300 Figures." A note at the foot of the title states that "Any Person may have this First Volume compleat, or any odd Months to perfect their Sets, and so continue them Monthly, and be enter'd a Subscriber when finish'd." An explanation of the last phrase is to be found in the preface where the printing of a list of subscribers is promised for the final volume. This promise does not seem to have been carried out, and the phrase is dropped from the similar note in the second and third volumes, where the reader is informed that the price of each part was 1*s*.0. The second volume "Contains 13 Months, and finishes whatever relates to the Muscles, Veins, Arteries, Nerves, &c. With the Three Venters, viz. the Head, Breast and Belly; the Diseases belonging to them, and the Parts contained therein; with their Cures: Collected and Extracted from several scarce Tracts, not in English before; from which, and other the best Anatomical Authors, are taken the most accurate Figures that are answerable to the Design." The third and last volume "Contains 13 Months, and finishes whatever relates to the Blood, Fluids, Motion of the Heart, Chilification, Circulation, of Poisons; of the Diseases and Cures in the Genital Parts of Men and Women, &c. as well as from Mr. Bidloo; Also the Surgeons Pharmacopoeia done into English; from Mr. Bolnest, Surgeon to the French King's Hospitals. This 3d Volume Collected and Extracted from 66 several scarce Tracts; as appears by the Contents, not in English before; from which, and other the best Anatomical Authors, are taken the most accurate Figures answerable to the Design."

It is on pages 25-34 of this third volume that the abridgement of the *De motu cordis* is to be found. At the top of page 25 is the section heading, "*Of the Motion of the* Heart, *and Circulation of the* Blood. *From* Harvey, Lower, Wallaeus, De Buck [*sic*], Ent, Bartholin, Baglivy, Verheyen, St. Hilaire, &c. *And first of* Harvey's *Circulation,* &c."

The abridgement is made not by rewriting but by omitting from the text passages not considered absolutely essential to the argument. With the omission of odd paragraphs here and there it corresponds closely with chapters II - XIV of the original and seems to be based on, but not identical with, the 1653 translation which was reprinted in 1673. The opening passage which I quote here should be compared with that of the earlier publication, either in the original or in Sir Geoffrey Keynes's Tercentenary edition published by the Nonesuch Press in 1928.

The Heart of all Creatures being dissected whilst they are yet alive, by opening the Breast, and cutting open the *Capsula* or *Pericardium* that incloses the Heart, you may observe that it sometimes moves, and sometimes rests. This is evident in the Hearts of colder Creatures, as *Toads, Serpents,*

Frogs, Crevises, &c. but it shows itself more manifestly in the *Hearts* of hotter Animals, as *Dogs, Swine.* If you observe diligently while the Heart is dying, and moves faintly, then you may plainly see that the Motions are slower and seldomer, and the Restings of longer Continuance; and you may distinguish easily what Manner of Motion it is, and which Way it is made.

The publisher gives us no indication of the person responsible for this version. In the preface of the third volume we are told that "They were translated or abstracted by several Hands; which, we hope, are done with Care and Exactness, as the Reader will find."

John Nutt, the printer, and John Morphew, the bookseller, seem to have been the leading spirits in this venture, for to them were to be sent suggestions of titles of tracts for inclusion. Nutt had started his career as printer, spent several years as bookseller-publisher (during which time his most famous publication was Swift's *Tale of a Tub,* 1704), and later returned to printing. From the few copies of this work which have come down to us it seems that it could not have been a commercial success, and it is possible that quantities of unsold sheets may have been pulped.

F. N. L. POYNTER

Rare Prescription Book of Frontier Minnesota•

The discovery of a rare prescription book, kept by a pioneer Minnesota druggist who was an early associate of Dr. W. W. Mayo, sheds new light upon the history of pharmacy and medicine in the Northwest. Indeed, this collection is one of the few available sources which establishes beyond doubt the drugs in general use in Rochester and the surrounding countryside during the period after the Civil War when Dr. Mayo was establishing the practice which led eventually to the formation of the world-renowned Mayo Clinic.

The huge, calf-bound volume, 21½ by 19 inches and containing some 150 pages, has about 5,274 prescriptions pasted in. The first is dated September 29, 1866; the last 1,667 prescriptions bear no dates. After August 1870 the formulas appear without the names of the physicians who wrote them. Nevertheless, the prescriptions of most of the early physicians of Rochester and of Olmstead County are represented, including Dr. E. M. Roys, the third woman to practise in Rochester, but apparently the first who was a regular member of the profession. Originally used by Dr. Ole W. Anderson, this book, together with supporting documents, was lent to the authors by Mrs. Elmer A. Haugen and her son of Rochester.

Dr. Anderson emigrated to this country from Norway when about eight years of age, settled briefly in Wisconsin, and then journeyed to southern

• The authors are indebted to Dr. Elizabeth M. Cranston, Assistant Professor of Pharmacology in the University of Minnesota, for her assistance in the preparation of this article.

Minnesota. A tall, good-looking gentleman who always wore a full beard, he opened a shop next to Head's Stationery Store in Rochester during 1866—perhaps in September and certainly not a year later as is sometimes said. He was then twenty-six years of age, having been born near Bergen on 22 May 1840. His pharmacy contained, in addition to drugs, a variety of patent medicines, perfumes, toilet goods, surgical instruments, and a stock of wines and liquors for medicinal purposes. Anderson himself was a staunch prohibitionist.

Probably in December 1867, although it may have been earlier, Anderson and Mayo entered into an informal partnership which continued for about three years. Mayo made Anderson's well-stocked shop his headquarters, and most of his prescriptions seem to have been filled there. Curiously enough, the standard biography of the Mayos makes no reference to Anderson or to his early relationship with Mayo. Indeed the name of Ole W. Anderson appears nowhere in Helen Clapesattle's *The Doctors Mayo*. Yet Anderson was the outstanding pharmacist in Rochester, and Mayo leaned heavily upon him. This is attested by the fact that the majority of the early prescriptions in the volume are written on printed blanks reading: "From Dr. Mayo. Third Street, Rochester." Other blanks are stamped in ink with the same legend. Anderson, of course, used his own blanks. These were imprinted, in addition to the name, with a mortar and pestle, a caduceus, and a small drug scale. Apparently Anderson furnished these to physicians in the area, for with the exception of Mayo and one or two others, few prescriptions were written by physicians on blanks of their own.

It is entirely possible that Anderson has not entered more into the medical history of Olmstead County because he was better known as a pharmacist than as a physician. Indeed, as far as can be ascertained, he never received any formal training in medicine. He did, however, receive a state license to practise after the Minnesota medical regulatory act of 1883 was passed. Anderson's license was granted on the basis of professional knowledge and years of experience. It is possible also that a later shift in activities carried Anderson farther and farther away from close contact with the profession.

Beginning about 1870, Anderson, while still maintaining his drug shop, began compounding and retailing a variety of home remedies. Called "Norwegian Family Medicines," they included such items as Hysteric Drops, Elixir of Life, Blood and Liver Renovator, and Compound Syrup of Blackberry Root. These were put up with labels in Norwegian and in English. Anderson's order books and ledgers, which we have examined, indicate that he was very successful with these remedies. They were well spoken of and were in demand even after his death in Rochester on 26 December 1920.

The prescription book makes reference to a vast number of preparations which cover practically the entire pharmacopoeia of the period. Certain prescriptions, of course, were more popular than others. It is hardly neces-

sary to say that there is no mention of synthetic drugs during the period after the Civil War. The principal minerals in use during Anderson's day were the mercurials, iodines, iron, arsenic, antimony, and sulphur and lead, although it appears that the rather heroic dose of earlier days had gone out of fashion.

Among the vegetable drugs, the opiates took the leading place as these were the only drugs relied upon for the relief of pain. Other outstanding vegetable drugs in constant use were quinine, ipecac, digitalis, squill, ergot, veratrum, aconite, and nux vomica. Types of preparations used were, of course, extracts, tinctures, decoctions, oils, and essences.

The mercurials were used internally and externally and were represented by calomel, mercury bichloride, and strong mercurial ointment. Those for internal use often were combined with ipecac, quinine sulphate, or tincture of veratrum viride. Mercury bichloride was diluted with water, glycerin, and syrup for internal use. One prescription called for mercury bichloride, ammonium chloride, acetic acid, tincture of nux vomica, and water. Mercurial ointment was applied in combination with morphine and extract of belladonna. A popular mercurial was Donovan's Solution or *Liquor Arsenici et Hydrargyri Iodidi.* This no doubt was used as an "alterative" in various chronic skin diseases such as psoriasis, lupus, pityriasis, and luetic lesions.

Prescriptions most frequently found in the volume contained iodides and iron, either alone or combined. The prevalence of chronic ailments and the long duration of severe acute diseases on the frontier often resulted in prolonged periods of convalescence for patients. The tonic effects of iodides and iron were desired in such conditions. Iodine usually was used as the iron or potassium salt. Syrup of ferrous iodide and tincture of iodine were the most frequent preparations of this nature. The latter occasionally was used internally (a few drops in sugar water three times a day), but more frequently it was prescribed for external use, not as a disinfectant but as an irritant, and was often combined with camphorated oil.

Iron was prescribed in many forms and combinations. The most popular prescription called for syrup of ferrous iodide. Other commonly used preparations were ferric citrate, soluble ferric pyrophosphate, tincture of ferric chloride, ferrous sulphate, iron and potassium tartrate, and tincture of cinchona and iron. Other drugs added to these included quinine, strychnine, tincture of cantharides, and occasionally morphine or laudanum. A popular patent medicine of the day was Nichol's Elixir of Bark and Iron. A pleasant prescription, found in the volume, provided ferric citrate in Jamaica rum and syrup.

Arsenic was prescribed frequently in the form of Donovan's Solution and less often as Fowler's Solution. Silver was often used as a solution of silver nitrate and very occasionally as "Argenti Nitras Stick, one inch." Its internal use was common, particularly in combination with bismuth subnitrate, opium, or gum arabic.

The giant collection in Anderson's book shows that quinine and the opiates were the most commonly used drugs. The latter was used alone or, more frequently, added to various mixtures. Dover's Powder was extremely popular, as were laudanum, paregoric, pills of opium, and pills of kino with opium. Combinations of powdered opium with silver nitrate and bismuth subnitrate, or Dover's Powder with quinine and capsicum were prescribed. Paregoric was ordered alone or in combinations with chloric ether, with tincture of cinchona and iron and chloric ether, with extract of ipecac and syrup of tolu, with tincture of lobelia and syrup of squill, with extract of aconite, or with sweet spirit of niter. Morphine was used as frequently as opium. A favorite prescription combined morphine with quinine sulphate, capsicum, and ipecac. In other prescriptions morphine was combined with iodides, mercurials, and quinine. For dysentery, morphine was combined with tannin.

The use of quinine was prodigious. It was ordered mainly as quinine sulphate, tincture of cinchona and iron, or as Peruvian bark. Favorite prescriptions combined quinine with mercurials and ipecac, with iodides, with morphine or opium in various forms, and with chloric ether. Quinine's main use, of course, was in the treatment of malaria (intermittent fever or the ague), which was still prevalent in the Minnesota area. It was also given for its tonic and "indirect sedative" properties. One prescription for a tonic contained quinine sulphate, ferrous sulphate, morphine sulphate, arsenous acid, and extract of nux vomica.

Belladonna was used mainly externally as belladonna plaster or as extract of belladonna "to be spread upon adhesive plaster," no doubt as an anodyne.

Many prescriptions called for digitalis, for it was one of the most important and most frequently used drugs. Digitalis, however, usually was only one of many ingredients called for in a prescription, as, for example: *tinctura opii camphorata, tinctura sanguinariae, tinctura hyoscyami, tinctura digitalis, extractum senegae, syrup simplex, tinctura benzoini composita,* and *extractum cubebae.* Or the tincture was combined with tincture of cinchona and iron or chloric ether or tincture of cantharides. This last combination (mixed in equal parts) was given in the following dose: "20 drops three times a day, increase to four times a day and to 30 drops if not improving in 3 days."

Other frequently used vegetable drugs were veratrum, usually as tincture of veratrum viride and aconite in the form of its extract or tincture. Aconite was combined with tincture of opium, extract of hyoscyamus, or spirit of niter. Veratrum was used mainly for its pulse-reducing qualities in febrile and inflammatory conditions. Aconite, too, was supposed to have antiphlogistic properties.

Strychnine was used for its tonic effect and dispensed as strychnia (no doubt strychnine sulphate) or as tincture of nux vomica. One prescription, it is interesting to note, contained ammonium chloride, strychnine, diluted

hydrochloric acid, extract of senna, and extract of taraxacum (dandelion extract). Other combinations were formed with citric acid, acetic acid, or ferric pyrophosphate.

One preparation which must puzzle those not familiar with the older pharmacopoeias is chloric ether. It was often called for in this collection of prescriptions. Sometimes it was combined with paregoric or with tincture of cinchona and iron. Chloric ether, whose official name is *Spiritus Chloroformi,* is a mixture of chloroform and alcohol. It was used by the medical profession for two reasons: first, it was easier to incorporate in mixtures than was pure chloroform, and second, the alcohol was considered an antagonist to the depressing effect of the chloroform. One prescription, in which chloric ether was diluted with an equal amount of tincture of gentian, mentions the condition for which the preparation is indicated, "A teaspoonful when stomach is feeling badly." Chloroform is called for several times, both alone and in mixtures. Spirit of nitrous ether (spirit of ethyl nitrite or sweet spirit of niter) was a highly esteemed drug of the nineteenth century. It was often mixed with equal parts of spirit of juniper, spirit of terebinth (oil of turpentine), and glycerin or combined with dilute nitric acid, syrup, and water. Sweet spirit of niter was used for many afflictions, being considered a good diaphoretic and diuretic and helpful in nausea and flatulence.

That prescriptions to be used for the treatment of constipation were plentiful is to be expected. Aloe was prescribed as *Decoctum Aloes Compositum* or *Pulvis Aloes,* the latter often being combined with *Resina Podophylii.* This must have been a powerful cathartic. The same, of course, can be said of croton oil, which was given with castile soap or alone in pill form. Citrate of magnesia was used frequently, but Epsom Salts apparently only rarely. Compound cathartic pills, an official preparation of long standing, contained colocynth, jalap, calomel, and gamboge. Some prescriptions called for "improved" cathartic pills and hepatic pills, which probably resembled later pills of the same designation. It is rather surprising that castor oil was not called for more frequently in Anderson's prescription book. One prescription, however, did call for two ounces of castor oil and fifteen drops of spirit of turpentine.

Dysenteries must have been common and severe in the Minnesota country. They were treated with *Mistura Cretae* (chalk mixture) combined with opium and kino, a powerful astringent, or with opium and *Vinum Catechu.* Tannin was frequently used alone or in combination with morphine. One prescription, containing tannic acid, zinc sulphate, carbolic acid, and water, was to be used "morning and night as an injection." Probably this was prescribed for an ulcerative or hemorrhagic condition of the rectum or colon, although it is not impossible that a foul, chronic fistula was to be irrigated.

The volume contains many prescriptions for external application. Tincture of iodine has been mentioned. Cantharis was often prescribed as cantharides plaster. "Liquid blister to be put on the feet when they are

swollen" may refer to *Linimentum Cantharidis* or *Collodium cum Cantharides,* which was known popularly as "blistering collodium." Another irritant ordered frequently was croton oil or *Oleum Tiglii,* usually diluted with camphorated oil. Milder rubefacients were volatile liniment (ammonia liniment), which was used alone or with oil of origanum, which in itself is a mild external stimulant, and ammonia water with camphorated tincture of soap and oil of wormwood. This preparation was to be "rubbed on the parts." At times, camphorated oil and oil of juniper were added.

Prescriptions are found also for afflictions of the ears, eyes, and throat. Carbolic acid in glycerin and water was used as eardrops. Other prescriptions refer to an ophthalmic balsam and to eye lotions compounded of tincture of opium, glycerin, zinc sulphate, and sugar of lead (Lead Acetate).

Taken together, the many prescriptions in this fat old tome not only reveal the state of health on the Minnesota frontier but also demonstrate the composition of an unusually large number of formulas, each of which, no doubt, served a definite purpose. There is something romantic also in the very names of many, now almost-forgotten, tinctures, extracts, oils, and balsams. Apothecary shops of nearly a century ago, crowded with handsome jars and sparkling vials in various shapes and sizes, must have pleased both the eye and the olfactory sense. The prescriptions in Dr. Anderson's volume conjure up the fragrance of the essences of mint, of lavender, juniper, camphor, anise, lemon, marjoram, and dozens of others. The prescriptions of the nineteenth century may have been less efficacious than those now written, but the writing of them a hundred years ago was indeed an art.

PHILIP D. JORDAN AND ROBERT ROSENTHAL

John Dalton's Autopsy

Three years ago in this journal (1954, *9,* 360-362) Denis I. Duveen and Herbert S. Klickstein published a note regarding the autopsy of John Dalton, the Englishman noted in the physical sciences for the concept of the atomic theory and in the biological sciences for his pioneer study of color blindness or Daltonism.

Dalton himself was afflicted with color blindness. He first noticed his optical peculiarity in 1790 when he was studying botany and discovered that he had difficulty in distinguishing the colors of flowers. He then made a systematic study of the phenomenon and in 1794 and 1795 read papers on the subject before the Manchester Literary Society. After his death in July 1844, at the age of seventy-eight, the physician Joseph A. Ransome removed Dalton's eyes and examined them to ascertain if there was some abnormality in the retina or humor that would explain his inability to distinguish certain colors. Ransome's observations, written in a letter to Dalton's biographer William Henry, were reprinted in the note of Duveen and Klickstein, referred to above.

Recently in the *Memoirs and Correspondence of Lyon Playfair,* edited by Sir Wemyss Reid (New York and London, Harper, 1899, p. 58), I came across additional information concerning the *modus operandi* of the examination. It was in the laboratory of Lyon Playfair that at least a number of the experiments were carried out. Playfair at the time was lecturing on chemistry at the Manchester Institute; later he became professor of chemistry at Edinburgh and tutor to the Prince of Wales and was much relied on by the Crown for advice in public affairs related to science. The aged Dalton had been a frequent visitor at the lectures of Playfair, a fact which the young teacher modestly explained by the supposition that organic chemistry was unfamiliar to the old philosopher, and Playfair was acquainted with Dalton's optical abnormality. In his *Memoirs* Playfair gave the following account of the examination of Dalton's eye:

> It is well known that Dalton was colour-blind, and he was the first person to investigate this defect of vision. He always ascribed it to a peculiarity of structure in the retina. When he died his medical man, Mr. Ransome, took out one of his eyes and brought it to my laboratory. I took two powders, chrome green and scarlet potassic biochromate, as being the colours which he could not distinguish, but we saw them of the natural colours when Dalton's eye intervened. Ransome, who was a most accomplished physician and a great friend of Dalton, assured me that the philosopher, when alive, would have approved of this experiment being made at his death.

It is odd that Ransome did not mention the assistance he received from Playfair. In the letter to Henry, quoted by Duveen and Klickstein, he speaks as though he alone conducted the examination.

WYNDHAM D. MILES

William Harvey's Diploma from Padua, 1602

In 1908 the original Diploma of Doctor of Medicine conferred by the University of Padua upon William Harvey was reproduced by the Royal College of Physicians. The facsimile reproduction was accompanied by a scholarly pamphlet by J. F. Payne,[1] the Harveian librarian of that institution.

There is one point to comment upon. The diploma is granted in the name of "Sigismund de Capilisti, of Padua, Count of the Sacred Palace of the Lateran, and of the Court of the Caesars, and of the Imperial Consistory, etc." (Payne, p. 13). Castiglioni[2] spells the name Sigismondo Capodilista. This nobleman, however, "was not the Chancellor, nor does he describe himself as Rector, or use any other official title" (Payne, p. 7). This

[1] Payne, J. F. *Notes to accompany a facsimile reproduction of the diploma of doctor of medicine granted by the University of Padua to William Harvey 1602,* with a translation. London, privately printed at the Chiswick Press, 1908.

puzzle may be solved by the following historical facts chiefly derived from Castiglioni. Harvey was non-Catholic. In his case the diploma could not have been granted by the "Sanctum Collegium" presided over by the local Bishop as Chancellor of the University. Besides, Pius IV in his bull of 1565 ruled that the degree could be given only to those of the Catholic faith. Then the Venetian Senate, to whose jurisdiction the University of Padua belonged, "named a Procurator who had the power to grant the degree without regard to the faith of the candidate. . . . In 1589 such a diploma was given to the Jewish physician Salomone Lotio by the Count Sigismondo Capodilista, the same Count Palatine who in 1602 conferred the degree on William Harvey."[2] Omission of "any testimonial of orthodoxy or confession of Faith . . . shows the tolerant and liberal spirit characteristic of Padua" (Payne, p. 8).

The nonecclesiastic form of Diploma at Padua made it possible especially for Jewish students to graduate from north-Italian universities. This point has been excellently described and documented by the distinguished historian Harry Friedenwald[3] of Baltimore.

Harvey's diploma was signed also by a syndic, a notary public, and four professors of Padua, among them Hieronymus Fabricius of Aquapendente. One of these "did there solemnly decorate and adorn the same noble William Harvey . . . with the accustomed Insignia and ornaments belonging to a Doctor: For he delivered to him certain books of Philosophy and of Medicine, first closed, and then, a little while after, open; he put a golden ring on his finger, he placed on his head the cap of a Doctor, as an emblem of the Crown of Virtue, and bestowed on him the Kiss of Peace with the Magistral Benediction" (Payne, p. 16).

A similar ceremony is described in the "Oration of Caelius Calcagninus[4] on (Granting) the Doctorate to Ruben, the Hebrew." The tradition of the insignia and ornaments[5] has survived to some extent to our time. When during a visit under Dr. and Mrs. Fulton's hospitable roof I told the invited guests of the curious passages in Harvey's diploma, Dr. Fulton genially "put a golden ring on his finger and placed on his head the cap of a Doctor," both bestowed on him recently at an academic ceremony at Uppsala. Thus the founder of physiology and an eminent physiologist and historian in modern times have been equally decorated by the insignia of "true spiritual nobility" and "crowned by . . . studies" (Calcagninus).

JOSHUA O. LEIBOWITZ

[2] Castiglioni, Arturo. *A history of medicine,* translated by E. B. Krumbhaar, 2d edition, New York, 1947, pp. 487-8.

[3] Friedenwald, Harry. *The Jews and medicine.* Baltimore, 1944, Vol. 1, Essays XIV, XVI, and XVII.

[4] Calcagninus, Caelius. *Opera aliquot,* Basel, Froben, 1544, p. 556. (Photostat and translation of p. 556 in Friedenwald, pp. 259-261).

[5] A like ceremony as held at Salerno is described in the Statutes of its College. Cf. Freind, J. *The history of physick,* 4th edition. London, 1750, Part II, p. 230.

The Love-Sick Patient in the History of Medicine

A simple, brief story about a love-sick patient threads its way through the history of medicine. As it reappears at various times it presents striking similarities of detail. It seems to acquire a legendary quality. For this reason, whether basically factual on any one occasion or essentially a fable on several, it may well be considered a part of the history of psychiatry and of psychosomatic medicine.

Erasistratus, anatomist, physiologist, and clinician, is said to have been called to the bedside of Antiochus (324-262 or 261 B.C.), son of King Seleucus I (Nicator), for the purpose of relieving his melancholia. As described by Bettmann (*A Pictorial History of Medicine,* Springfield, 1956, p. 31) and also by E. N. R. Grigg (*J. Hist. Med.,* 1955, *10,* 74-5), the physician palpated his patient's pulse as the women of the court passed by. Its quickening led to the diagnosis. The reaction of Antiochus was to Stratonice, his stepmother, and, as the story is related, Erasistratus persuaded the monarch to divorce his wife so that Antiochus might marry her. The story is told by Valerius Maximus 5. 7. ext. 1 and by Plutarch in his life of Demetrius, 38. Galen (Kühn 14.630, 18B.18) mentions it with some reservations, and there are certainly chronological difficulties. Pliny 7.123 assigns the diagnosis not to Erasistratus but to his father Cleombrotus.

In his *History of Medicine* (Springfield, 1954, p. 199) Major makes a brief reference to a similar story about Galen (A.D. 129-?199). A depressed patient revealed her problem when her pulse rate increased at the mention of the dancer Pylades. Galen himself relates (Kühn 14.631 ff.) that he was called to the bedside of a melancholic lady who evaded his searching efforts to evaluate her difficulty. Managing to be near her after several rebuffs, he observed an increase in her pulse rate apparently in specific relation to the mention of Pylades. Major refers to Galen as skilled in psychosomatic medicine.

The third and final example of this story form is given by Zilboorg (*A History of Medical Psychology,* New York, 1941, p. 267 as quoted from René Semelaigne, *Les pionniers de la psychiatrie française,* vol. 1, 1930, p. 47). The physician was Jacques Ferrand, and he was to write afterward a treatise *De la maladie d'amour, ou mélancholie érotique* (Paris, chez Denis Moreau, 1623). Beginning on page 92 of this book, and after referring to such stories as those concerning Stratonice and Pylades, Ferrand relates that in May, 1604, he called to see a depressed young man whom he had known previously as cheerful and energetic. When pallor and an irregular pulse were detected in the patient at the time a particular young woman was present, his doctor sensed the underlying problem. Ferrand took his patient in charge, seeking a solution in the face of parental objections to marriage for this couple. Thereafter, similar problems were of great interest to Ferrand. Ascribing considerable intuitive insight to him and the beginnings of an impressive psychiatric attitude, Zilboorg stresses his apparent

realization of the significance of awareness on the part of emotionally sick patients of their own illness. Recognition of this type of "insight" is one of the steps in the development of psychiatric thought and practice.

It is curious that this little anecdote, repeated from time to time, seems to appear in modern times as an isolated reference. The data should be integrated as I had integrated them even without the primary sources, and as I then found Ferrand had already done. There is an obvious resemblance between his experience and that of Erasistratus and Galen.

JEROME M. SCHNECK

Two Early Surgeons

On 25 October 1956, Dr. William Brockbank of Manchester, England, delivered the Thomas Vicary Lecture at the Royal College of Surgeons of England. He presented under the title "The Man Who Was Vidius" a biographical study of the Florentine surgeon Guido Guidi, with a critical assessment of his *Chirurgia, De Anatome,* and other works. The lecture has been printed in *Annals of the Royal College of Surgeons of England,* 1956, *19,* 269-295; it is freely illustrated mainly with woodcuts reproduced from the *Chirurgia.* From baptismal registers the date of Guidi's birth can be fixed as 10 February 1509 by our reckoning. He died in 1569. Dr. Brockbank concludes that he was a distinguished surgeon, physician, and anatomist, a brilliant teacher, a classical scholar, a practical man rather than a theorist, and a man who enriched anatomy with original discoveries and medical literature with a particularly beautiful book.

A similar examination of the contribution of a German surgeon is given by Dr. Paul Tower in his "Notes on the Life and Work of George Bartisch," which appeared in the *Archives of Ophthalmology,* 1956, *56,* 57-70. Readers familiar with the *Ophthalmodouleia* (Dresden, 1583) of Bartisch will be grateful for this survey of the life and career of its author, an itinerant physician who helped to sow the seeds of modern ophthalmology. His book appeared in a second edition in 1584 and again more than a hundred years later, in 1686.

Edward Hicks Hume, 1876-1957

The JOURNAL records with deep regret the death, on 9 February 1957, of Dr. Edward Hicks Hume, who in a crowded life of service to modern medicine took the time and trouble to cultivate the history of his profession. Born in India, he returned there in 1903 after receiving his medical degree at Johns Hopkins. In 1904 he helped to organize Yale-in-China, and for more than twenty years thereafter he headed its medical service. His policy during this period and in a later residence in the Orient in 1934-37 stressed partnership with the Chinese in medical education. He was a director of the New York Post-Graduate Medical School, director of the Christian

Medical Council for Overseas Work, and Lecturer in the History of Medicine at the Institute of the History of Medicine of the Johns Hopkins University. His articles and book reviews in the field of medical history are numerous; his last paper before the American Association of the History of Medicine was delivered in Columbus on 11 April 1953 and examined the history of variolation in Africa and Asia. One of his books, *Doctors East, Doctors West,* which appeared in 1946, received the Norton Medical Award as the best book of that year written by a professional man for the layman.

Dr. Hume was a missionary in the finest sense of the word. An editorial in *The New York Times* of 11 February 1957 emphasizes the abiding value of his work in the East. On 12 February 1957 a memorial service was held for him in Dwight Hall of Yale University.

Dr. Edward B. Krumbhaar Honored

On 12 February 1957 at a meeting of the Section on Medical History of the College of Physicians of Philadelphia, Dr. Herbert J. Dietrich, Jr., Fellow of the College of Physicians, reviewed the life of John Lawrence and discussed his "Oration on the Birthday of Medical Honors in America," the first commencement address at what is now the Medical School of the University of Pennsylvania. This Latin manuscript, miscatalogued for many years as a thesis, was recently discovered by Dr. Walton B. McDaniel 2d in the process of moving into the new stacks at the College.

This paper was followed by one entitled "John Redman: Medical Preceptor of Philadelphia" given by Dr. Whitfield J. Bell, Jr., Associate Editor of *The Papers of Benjamin Franklin.*

In recognition of Dr. Edward B. Krumbhaar's outstanding contributions to the field of medical history, the Section has named its medical history essay prize in his honor, and on this occasion Dr. Krumbhaar himself presented the first award to Dr. Marshall Smith. The contest is open to the students of Philadelphia's five medical schools. Dr. Smith, a 1956 graduate in medicine of the University of Pennsylvania, received a certificate and one hundred dollars for his essay entitled "Leonardo da Vinci and Vesalius—a Comparison." Dr. Stuart H. Shippey, Jr., a 1956 graduate of the same school, received honorable mention for his "Biographies of Professors of Medicine at the University of Pennsylvania." These papers will be published in the *Transactions and Studies* of the College of Physicians of Philadelphia.

Dr. Samuel X Radbill paid tribute to Dr. Krumbhaar's work in pathology and medical history.

Sesquicentennial of the Medical Society of the State of New York

On 3 February 1807, when the State of New York had 736 practising physicians, its Medical Society held its first meeting at the City Hall in Albany. Practising physicians in New York now number 29,934, and their

Society, among medical groups, is surpassed in membership only by the American Medical Association and the British Medical Association. But it was influence and attainments, not size, which were celebrated in the sesquicentennial held in New York City during the week of 18 February 1957.

An excellent History of the Medical Society of the State of New York, 1807-1957, was aptly provided in the *New York State Journal of Medicine*, 57, no. 3, 1 February 1957, pages 433-532, by Norman Shaftel of Brooklyn for "The First Seventy-Five Years"; Emerson Crosby Kelly of Albany for "The Years Between, 1882-1906"; and John F. Rogers of Poughkeepsie for "The Last Fifty Years," the progress during which indicates that in another sense they cannot be the last. The survey is copiously illustrated and in its 100 pages (which should have been 150?) traces vividly the development of the Society and of medicine in the State of New York.

The New York Times for 24 February carried a competent review by Howard A. Rusk of the Society's contributions and of the problems still ahead. General interest in the celebration was evoked also by a device reported by Meyer Berger in the issue of 18 February, on which day the last horse-drawn ambulance in the City of New York rolled again through midtown streets (at a cost to the Society of $50 for the hire of one horse for three hours). It had been hoped that Emily Dunning Barringer, the Cornell graduate who in June, 1903, became the first woman interne to ride a hospital ambulance, might make this ride of 1957; now 80 and still very capable, she had expected to come down from Connecticut for the occasion, but was prevented at the last moment by a death in her family. Dr. Barringer served the ambulance of Gouverneur Hospital, which often raced the Henry Street engines to fires and other disasters. On the night her ambulance pulled alongside *and* ahead she received an enthusiastic salute from the outdistanced firemen.

D. J. Davis Lecture

The annual D. J. Davis Memorial Lecture on Medical History was delivered at 1:00 p.m. on 15 May 1957 at the Medical-Dental-Pharmacy Building of the University of Illinois College of Medicine, Chicago. The lecturer was Dr. Elmer Belt of Los Angeles. He spoke on the original observations of Leonardo da Vinci.

Annual Kate Hurd Mead Lecture
College of Physicians of Philadelphia

On Tuesday evening, 9 April 1957, Mr. Robert Scott Stevenson, F.R.C.S. Edin., of Gibraltar, United Kingdom, delivered the annual Kate Hurd Mead Lecture sponsored jointly by the College of Physicians of Philadelphia and the Woman's Medical College. Mr. Stevenson, the author of several books

on Harley Street, chose as the title for his lecture, "The Changing Scene in Harley Street."

Biography of Dr. Charles V. Chapin

Mr. James H. Cassedy is preparing a biography of Charles V. Chapin (1856-1941), the eminent Providence pioneer in the American public health movement. He writes: "Since many of [Chapin's] own files were destroyed by flood, I would very much appreciate hearing from any of your readers who have personal recollections of Dr. Chapin or who know the location of any of his correspondence." Mr. Cassedy's address is 69 Barnes Street, Providence 6, Rhode Island.

Henry E. Sigerist 1891-1957

The death of Henry E. Sigerist at his villa, Casa Serena, in Pura, Switzerland, on 17 March brought a deep sense of loss to his friends all over the world and especially to the Board of Editors of this *Journal* of which he was a member. Commemoration of his life and work will appear in a future issue.

Book Reviews

BROOKE HINDLE. *The Pursuit of Science in Revolutionary America 1735-1789.* Chapel Hill, The University of North Carolina Press, 1956. xl, 410 pp. $7.50.

Reviewed by FREDERICK G. KILGOUR, *Librarian, Yale Medical Library.*

THIS book is an excellent and unique work; it is the first book that discusses scientific activity throughout the British Colonies in North America in any period. Colonial science is an important topic because the roots of the American scientific tradition lie in this era. No real understanding of American science can be attained without having a knowledge of science in colonial America. Professor Hindle's book supplies that knowledge, at least from 1735 on, and herein lies its great importance.

The subject matter is by no means a chronology of discoverers and discoveries; rather it is a truly historical work which integrates colonial scientific activity with other activities. Moreover, Professor Hindle has not employed a restricted definition of "science" but has conducted his investigations from a broad point of view. He includes, for instance, discussions of surveying, the establishment and activities of scientific societies, the teaching of science, and thoroughly covers the medical history of the period.

The volume is a scholarly work. Professor Hindle has used original sources, including many manuscript collections, to a most commendable extent and has completely documented his statements. This scholarly apparatus further enhances the value of the volume; any future investigators of the history of colonial science or medicine in this period should begin with this book, for its extensive documentation will point the way to the initial steps to be taken.

It would appear that Professor Hindle's purpose was to present a broad historical analysis and the social relationships of scientific and medical activities of the later colonial period. He has admirably attained this goal and in so doing has made an outstanding and unique contribution.

The Surgery of Theodoric. Volume One (Books I and II). Translated from the Latin by Eldridge Campbell and James Colton. New York, Appleton-Century-Crofts, Inc. 1955. xl, 223 pp. $5.50.

Reviewed by GUSTAF E. LINDSKOG, *Professor of Surgery and Chairman of Department, Yale University School of Medicine.*

IN this English translation of Theodoric's *Chirurgia* from the 1498 and 1519 editions available in the National Medical Library, the late Dr. Eldridge Campbell and his colleague James Colton have given us an eminently read-

able and interesting account of early medieval surgical practices, and a revealing glimpse into the critically inquiring mind and character of the Dominican friar who wrote the original thirteenth century manuscript which Castiglioni characterized as the first awakening of modern surgery.

Theodoric (1205-98), bishop of Cervia, was a Bolognese, reputedly a Borgognoni and the son of Hugh de Lucca, of a distinguished family which gave several illustrious names to medicine. Indeed, our knowledge of Hugh de Lucca's work is largely through the writings and comments of Theodoric who was his professional disciple and chief protagonist. They were two pioneers who together dared stand against the established authority and ingrained errors of Roger, Roland Capelluti, and the Salernitan school. Not always in his writing does Theodoric demonstrate the Christian tolerance and forebearance consonant with his priestly status. Although repeatedly showing a proper deference for the ultimate authority of Galen, Avicenna, and Rhazes, he was, as the translators point out in their introductory commentary (which adds a great deal to the text), extremely disdainful, even contemptuous, of Roland and always disinclined to mention contemporaries or their attitudes toward matters of therapy. In Book II, chapter 17, under the title of "Recovery despite Amputation of Portion of Lung," Theodoric makes the following caustic remarks:

> Such a man as Roland commends himself to us; and having clothed himself in the fleece of another, strains to lengthen his fringes, because he cured someone without tearing away even a little bit of the lung. The truth of the matter is that, while I was living in Bologna, a certain Domicellus, a Bolognian of noble birth, was cured by the hand of Master Hugo, part of his lung being torn away, and Master Roland was present there and witnessed it.

It is in that large portion of the text concerned with head trauma and with fractures that Theodoric's knowledge is most impressive. Here his indebtedness to Hugh de Lucca is manifest and freely acknowledged. Theodoric had little patience for dogma and for conjecture divorced from personal experience. He is frank and explicit when the latter is wanting, as in Book I, chapter 17: "The two aforementioned procedures [concerning treatment of muscle wounds] recommended by experienced men are not approved by me; yet I have written them in this book, just as I shall write many others which I have received from those worthy of credence."

Perhaps at his best in the diagnosis and management of cranial trauma he recognized clinical tests for the detection of penetrating injuries and depressed fractures, and emphasized the need for rapid decompression and early removal of depressed bone fragments associated with local signs ("spasm," "apoplexy," Bk. II, ch. 6). He stated that serious depressed fractures could exist in the absence of an external wound. He had the courage to challenge the dictum that compound wounds were necessarily and uniformly fatal. He clearly stated the dangers of heavy sedation in those with

brain damage (Bk. II, ch. 5). His special interest in the head may have stemmed from a personal experience mentioned in Book II, chapter 7: "I know that I was thus cured, when I was a youth, of a serious cranial fracture, by nature expelling nine fragments of bone from the wound after some time had passed."

Genius has been defined as an infinite capacity for taking pains, and in this respect Theodoric certainly qualifies. "For let the physician make no mistake; as has already been said many times, he should be prudent and attentive to every detail." His instructions concerning the bandaging of wounds and fractures are meticulously detailed. An example (Bk. II, ch. 8) is his bolstering or padding of the two sides of a suture line after repairing a lacerated eyelid, a method still acceptable to the present-day surgeon.

Theodoric was gentle in the handling of tissues and, for the times, exceptionally considerate of the patient's comfort and welfare. To quote again: ". . . having stopped up the ears of the patient with cotton in order that he should not be bothered by the sound of scraping, or perforation or the hammer blow, . . ." (Bk. II, ch. 6)

In the management of nasal wounds he relates that ". . . with certain delicate patients who were not able to stand the closing of the nostrils [by plugging] because of the difficulty in breathing, I have inserted goose quills in place of the [gauze] rolls."

Ready to save a life when he could, Theodoric was not beyond prudence and caution. "And you should know, certain surgeons are overanxious to operate upon the wounded (Bk. II, ch. 5)." Dr. Frank Berry points out in his well-considered foreword to the volume that Theodoric really understood the concept of inoperability, when he wrote: "Before you proceed to the treatment of the patient, consider his symptoms and if what you see is obviously bad and seems likely to worsen, leave him alone." (Bk. II, ch. 2)

In the Dominican's introduction to the second book is written: "Starting with the head which is called the sovereign region because the five senses stem from it. . . ." With only the technical help and resources of these five senses, Hugo de Lucca and Fr. Theodoric evolved the concept of primary wound closure and per primum ("clean") wound healing, a thesis which required six centuries for eventual establishment. Pioneering again, Theodoric revived interest in the soporific sponges of the "ancients," and so anticipated inhalation anesthesia by a half millenium. These are accomplishments which deserve to be memorialized as Drs. Campbell and Colton have succeeded in doing. At the same time, the late Eldridge Campbell has given us another reason, if any be needed, why his own distinguished name will not soon be forgotten. It is to be hoped that the Appleton-Century-Crofts Company makes certain that the several additional books of the *Chirurgia* will ultimately be translated by an equally skillful and sympathetic team of authors.

OTTO L. BETTMANN. *A Pictorial History of Medicine.* With a Foreword by Philip S. Hench. Springfield, Illinois, Charles C Thomas, 1956. xiii, 318 pp. $9.50.

Reviewed by THOMAS R. FORBES, *Associate Professor of Anatomy, Yale University School of Medicine.*

BOOKS and magazines of pictures have a special fascination. *A Pictorial History of Medicine* is, in addition, concerned with a field in which the public is greatly interested, and the text and captions can be understood without difficulty by the layman. This volume, the first, I believe, of its kind, should quickly earn popularity.

The pictorial story is told chronologically. One or two pages of pictures and text are devoted to each of more than 150 topics. Among those subjects which are sketched with particular interest are transfusion, public health, quackery, and psychiatry. The material is clearly and vigorously presented.

Some illustrations are famous and familiar, others are obscure, and still others were drawn especially for this book. Almost all of them seize one's attention. Sources include books, manuscripts, photographs, paintings, and newspapers. The curiosity of many readers will not end when they close the volume, and for their sakes it is unfortunate that the origin of most of the pictures is not explained. The choice of illustration, on the whole, is good, although one may question whether the vulgarity of past centuries requires repeated proof. In general the reproduction of the originals is satisfactory; the quality of some is diminished by excessive reduction or poor printing.

Captions and text make it clear that the author has devoted much time to the history of medicine. (His major sources are listed in a bibliography.) The text is necessarily brief, but is often fascinating. Whether the facts, personalities, and anecdotes which Dr. Bettmann has selected for his sketches are always typical will be debated by some readers. The introduction by Dr. Philip S. Hench is enthusiastic and helpful.

Typographical and literary errors are minor but surprisingly frequent irritants. Even the comma, taken for granted when properly used, is as important as salt in food: badly needed in some things and quite unwanted in others. This book deserves better of its publisher. In spite of flaws, however, it tells a lively, entertaining, and informative story.

ARTHUR RUSKIN. *Classics in Arterial Hypertension.* Springfield, Illinois, Charles C Thomas, 1956. (Publication Number 290, American Lecture Series.) xxv, 358 pp., illus. $9.50.

Reviewed by RALPH H. MAJOR, *Professor of the History of Medicine, University of Kansas School of Medicine, Kansas City.*

THIS work should be of unusual interest to every student of the history of medicine and to every physician interested in the problem of hypertension. It is of a convenient format and is an attractive volume which tempts one to

read it as soon as he has it in his hands. The selections are well chosen and include certainly the outstanding discoveries which mark the progress of our knowledge of hypertension.

The old question could be raised that the first name of Galen or Galenos was not Claudius. The name of Claudius was apparently a medieval addition, which is not justified but which stems from the error of interpreting Cl. Galen as Claudius instead of Clarissimus. This reviewer thinks also that it would add to the interest of the book if the author had included some tracings made by Vierordt's sphygmograph, which is illustrated on page 82. The contribution of Vierordt in making the first pulse tracings should be stressed more than it has been. The volume would also be improved by an adequate index.

Classics in Arterial Hypertension is highly recommended to the serious student of hypertension.

G. A. LINDEBOOM. *Boerhaave's Brieven aan Bassand.* Haarlem, De Erven F. Bohn N. V. 1956. Translation from the Latin into Dutch. 273 pp.

Reviewed by A. E. D. J. HELLEGERS, *Josiah Macy, Jr. Foundation Research Fellow, Department of Physiology, Yale University School of Medicine.*

BOERHAAVE (1668-1738), considered by many to have been the father of modern-day clinical teaching, began his student days in the field of theology. He soon left it to move into scientific fields. Although usually remembered for his tenure in the Chair of Theoretical and Practical Medicine at the University of Leyden, it is perhaps less universally known that he also held the Chairs of Botany and Chemistry. The present volume contains ninety-four of his letters addressed to his former pupil Joannes Baptista Bassand (1680-1742), court physician to Emperor Charles in Vienna and himself greatly interested in Botany. The letters, discovered in the Imperial Library, are incomplete and represent only Boerhaave's side of the correspondence; Bassand's letters have never been found. Other translations of the letters contained in this volume are in existence, some appearing as early as 1750 among the *Consultationes medicae cum responsis Boerhaavianis,* edited in Paris and reprinted in Venice in 1753. These letters contain no mention of the addressee. In 1778 an unknown person edited the complete available series of letters under the title *Hermanni Boerhaave Epistolae ad Joannem Baptistam Bassand, medicum Caesareum Vindobonnae.* In 1781 Nusch edited a German translation of the letters, and some of these letters in the Nusch translation were edited, with an introduction, by E. Darmstaedter in 1927. Until the present volume only fragments of these letters had been published in Dutch—by Uuyens. Professor Lindeboom's translation into

Dutch is therefore long overdue, particularly when one considers Boerhaave's place in Dutch medicine.

Of the letters themselves one can say that they accurately reflect Boerhaave's life as one knows it. The largest part is devoted to botanical matters, although chemical problems play a gradually increasing rôle in the correspondence. Of the botanical matters dealt with, one gets a good idea of the importance attached by Boerhaave to the garden which he laid out in Leyden, and part of which graced his private domain. Long lists of requests for seeds and twigs are included in the letters, and one is impressed by the faithfulness with which Bassand acceded to these requests. To botanists these letters should shed an interesting light on the outline of Boerhaave's organization of his gardens.

Since chemistry had not yet reached the systemic scientific stage, the chemical matters touched upon are somewhat confusing to the present-day reader. They are nonetheless interesting in that one senses the magnitude of the task facing Boerhaave in his study of the subject, handicapped as he was by living in a country virtually devoid of minerals.

The greatest interest, however, centers on the many medical matters dealt with in the letters, consisting mainly of replies to questions raised by Bassand. Boerhaave had enormous experience in this form of written consultation, having been consulted widely from all parts of Europe, and having trained many of its foremost physicians. Dysentery and podagra are the subjects most frequently alluded to. Throughout the letters one senses the inclination to the viewing of the patient as a whole, without attempts to define specific diseases. This therefore shows a classical allegiance to the teachings of Hippocrates. One also senses, however, the beginnings of the search for specificity in disease, and one can visualize Boerhaave in the early eighteenth century vacillating between the teachings of Hippocrates and the as yet unpublished theories of Pinel and Trousseau. Throughout, there is, as could be expected from him, clear evidence of his teaching of careful history-taking and acute clinical observation. In the absence of an approach along specific lines one sees him as an early master of psychosomatic medicine, making full use of his authority in medical matters.

To his family life there are but few allusions. As his infirmity increased, one is struck by his obviously religious outlook on life, as befits one who started in theology. His descriptions of his own activities in such matters as horseback riding, and his ceaseless attempt to spur Bassand and his patients to exercise, testify to what must have been a cast-iron constitution and will power.

The letters contain a wealth of other interesting little details of his times—occasional three-month delays in arrival of mail (while on the other hand coaches to Vienna travelled every two or three days), allusions to customs duties, philosophical remarks on the trials and tribulations of court physicians, and alternating signs of great humility and of vanity.

For all those interested in Boerhaaviana, or in getting an outline of medicine in the days of transition between the nonspecific and specific approach to disease, this well-edited little tome can be recommended.

ABBOTT PAYSON USHER. *A History of Mechanical Inventions.* Revised edition. Cambridge, Mass., Harvard University Press, 1954. xi, 450 pp., 159 figs. $9.00.

Reviewed by I. BERNARD COHEN, *Professor of the History of Science, Harvard University.*

THIS is not a new book, but a revision of Professor A. P. Usher's classic work, originally published by McGraw-Hill in 1929. The present revision is especially welcome because it is the only work in English devoted specifically to this topic and has long since established itself as a standard reference work. The major revisions consist of four new introductory chapters, new material on the early history of clocks, on the history of textile techniques in Syria and China at the beginning of the Christian era, and on internal-combustion engines and gas turbines.

Following the initial chapters on the early history of the "pure applied mechanical sciences," and the mechanical equipment of pre-Christian antiquity, there are chapters on the development of water wheels and windmills, and of water clocks and mechanical clocks, to about A.D. 1500. Further chapters deal with Leonardo da Vinci, the invention of printing, the machinery of the textile industries to about 1800, the development of clocks and watches into instruments of precision in the era from 1500 to 1800, the production and application of power from about 1500 to 1830, the use of machine tools and the introduction of quantity production in the period from 1450 to 1850, and a final chapter on the production and distribution of power since 1832.

Although Professor Usher has attempted to bring his bibliography up to date, he quite frankly points out that he has not had occasion to make use of all this new material in revising the separate chapters. Hence, although the main outline remains sound, the critical student who wishes to be certain about some particular point of real importance must, in almost every case, check the rapidly growing literature on the history of technology.

Drawing heavily on the literature of anthropology, sociology, economic history, and particularly gestalt psychology, Professor Usher explores the place of technology in economic history, and presents an historical analysis of social change. One of the new chapters presents the author's mature reflections on the way in which novelty emerges in thought and in action.

This distinguished volume deserves a place of honor on the reference shelves of students of the history of medicine and the history of science.

AMERICAN FOUNDATION. *Medical Research. A Mid Century Survey. Vol. I. American Medical Research in Principle and Practice.* Vol. II. *Unsolved Clinical Problems in Biological Perspective.* Edited by Esther Everett Lape and Associates. Boston, Massachusetts, Little, Brown and Co., 1955. Vol. I, 765 pp., Vol. II, 740 pp. $15.00.

Reviewed by GEORGE ROSEN, *Professor and Head of the Department of Public Health Education, Columbia University, New York City.*

A little less than two decades ago, specifically in 1937, the American Foundation published two volumes reporting the results of a survey intended to elicit the opinions of medical men on the need for changes in the provision, distribution, and financing of medical care. At that time a number of respondents, particularly leaders in medical education, expressed the opinion "that these economic and social problems can be permanently solved only as they are related to solution of the even more challenging problem of extending and improving medical education, with consequent progressive improvement in the quality and quantity of medical care." And since the vitality of medical education and practice was considered to derive from continuing contact with medical research, it was felt that ultimately the best health insurance is to have and to support the right kind of medical research. Motivated by this thesis, the American Foundation undertook another survey, this time of American medical research. The results of the survey are presented in the two volumes under review.

The survey covers the period from 1940 to 1955, thus dealing with research during the Second World War and the decade following the war. It is divided into two sections. Volume one deals with the general principles and practice of research; the second volume moves from the general to the specific and examines nine unsolved problems of clinical medicine. It is no exaggeration to say that the 1,500 pages of this report offer an extraordinarily comprehensive picture of the character and condition of present-day medical research in the United States. Thus the first volume reviews successively medical research in the perspective of biological, chemical, physical, and mathematical science; current trends and problems in medical and biological research; the various agencies (universities, foundation, industry, government *et alia*) that sponsor, support, and conduct research and the problems of controlling and implementing the results of research. Volume two reviews the problems of cancer, infertility, arteriosclerosis, hypertension, rheumatic syndromes, tuberculosis, viruses, chronic alcoholism, and schizophrenia.

Clearly, no review can do full justice to so comprehensive a statement of the problems of medical research in terms of its many details and facets. There are, however, certain overarching aspects whose discussion will help to define more closely the achievement and limitation of the survey. In short, we are concerned here with certain basic premises and points of view.

The first premise underlying the survey is that medicine—research, teaching, practice—must be biologically oriented or it is nothing. Biological science provides the fundamental frame of reference within which medical activity occurs. At the same time biology as a science is related in a variety of ways to physics, chemistry, and mathematics, utilizing whatever indicated concepts, methods, and tools from these sciences. The fundamental truth of this premise is unquestionable, and one may even ask whether it is necessary to belabor this point. Nonetheless, the fact is that the layman is generally unaware of how much basic research has gone into the development of the wonder weapons of modern medicine; and in a survey intended to illustrate the need for support of basic research it is essential to have this premise pinned down in detail.

At the same time, the handling of this theme illustrates an important limitation of this report, namely, its relative indifference to a genetic analysis and understanding of the phenomenon with which it is concerned—medical research (this is a defect of all cross-sectional studies and is not specific to this one). While the authors of the report indicate here and there that certain trends present today extend back into the nineteenth century, for the most part the discussion is centered on the fifteen years covered by the survey. This in many instances makes it appear that nothing had been accomplished prior to 1940. The continuity of problems and trends between the past and present should have received much more emphasis. After all, the great scientific generalizations of the nineteenth century—the cell theory, evolution, conservation of energy—all had important repercussions in medical research and provided the basis upon which present-day work is based. In short, one may say that the survey lacks an adequate historical perspective, a judgment which can be documented with other examples.

Another limitation is the absence of a focused sociological orientation. In part, this derives from a "romantic" approach to the problem of research with its emphasis on the "carriers of the flame" and the laboratory. Research is, after all, a social institution, and if it is to be properly comprehended must be studied within the societal framework in which it exists and functions. Furthermore, as an institution it has developed in time so that historical analysis must be employed for further illumination. Finally, in reacting against the layman's tendency to gape at the wonders of scientific medicine as they present themselves in therapy, the authors of this survey tend almost to do the same thing in reverse with an emphasis on the genius in science—the carrier of the flame of pure research. One cannot, however, ignore the evidence of numerous identical discoveries on all levels of scientific work, even on that of the highest level of generalization, as witness the statement by Darwin and Wallace at the same time of the theory of evolution by natural selection. And one might point to numerous other examples. True, in discussing financing and organization of research some attention is given to the institutional aspect of research, but it remains unrelated to any basic framework.

This tendency is reflected also in the space devoted to social aspects of biological and medical research as compared to that given over to medical research in the perspective of biological, chemical, physical, and mathematical science. Apparently this position is related to the original premise from which the study started; the extension and provision of good medical care is dependent on the promotion and development of medical education and research and not so much on financing. In the opinion of this reviewer, this premise is fallacious because it creates a pseudo-problem. There is no doubt that basic research and properly trained physicians are needed if good medical care is to be provided. At the same time there is more than enough evidence to show that even our present knowledge and skilled personnel are not being utilized as efficiently and effectively as they might be to give good medical care because of financial and organizational difficulties. From the volumes of the Committee on the Cost of Medical Care in 1933 to the reports of the President's Commission on the Health Needs of the Nation in 1952 to the recent volume of the Health Information Foundation in 1956, this point emerges over and over again. The acquisition of knowledge and the training of competent physicians do not automatically guarantee the availability of high quality medical care. It would seem that the invisible hand of Adam Smith still hovers over this survey of medical research, even though it has for the most part vanished from economic theory.

In summary, any one interested in knowing what have been the main trends of American medical research between 1940 and 1955 will get an excellent panoramic view in the two volumes issued by the American Foundation. This is the raw material of history. However, if one wants a deeper understanding of the material presented, of the why and the wherefore, of how it got to be that way, the reader will have to look elsewhere or develop it for himself. For the historian of medicine it is fascinating reading, especially if he is interested in attempting to understand the psychology of the scientist and the physician who deal with medical problems in the laboratory.

ALEXANDRE KOYRÉ. *A Documentary History of the Problem of Fall from Kepler to Newton.* De motu Gravium Naturaliter Cadentium in Hypothesi Terrae Motae. Philadelphia, The American Philosophical Society, *Transactions of the American Philosophical Society,* 1955, *n.s. 45,* part 4, pp. 329-395.

Reviewed by I. BERNARD COHEN, *Professor of the History of Science, Harvard University.*

ANY work from the pen of Professor Koyré of the Ecole Pratique des Hautes Etudes, Sorbonne, must command the attention of all students of the history of scientific thought. Professor Koyré has that special quality of scholarly magic that enables him to endow every subject that he explores

with a general significance for the way in which scientific ideas develop. His previous studies have dealt with the German mystics of the sixteenth century, the work of Galileo and of Descartes, the nature of Plato's influence in the development of scientific thought, and, more recently, the nature and background of Newton's achievement. In each of these, Professor Koyré has given the subject a new perspective by delineating the background of the major problems and showing their significance in a totally novel and unexpected way.

The work under consideration here is devoted to one of the most crucial problems of the sixteenth and seventeenth centuries. Briefly stated, the question is the accommodation of the whole of physical science to the revolutionary implications of the Copernican system. If the earth is at rest, heavy bodies fall straight down and will strike the earth at a point directly below that from which they are released. But, if the earth is to move, with a daily rotation and an annual orbital motion around the sun, it is far from obvious that bodies should still continue to fall straight downward. From the time of antiquity to the seventeenth century, one of the stock arguments against the impossibility of a moving earth was the experience of bodies falling downward in a straight line. It would seem, on first inspection, that if a body were let drop from a moving object, say the mast of a ship, it would not strike the ship at a point directly underneath that from which it was dropped, but at a point somewhat behind, since the ship would have moved in the meanwhile. A ball thrown vertically upward from a moving ship, would appear to have a motion directly upward and directly downward with respect to the earth; since the ship would have moved in the interval, one would naturally expect that the ball would not strike the ship at the same point from which it was thrown up, but somewhat behind. Applied to the earth, the numerical results are startling. Suppose, at the equator, a rock is to be flung into the air to a height of about 150 feet. It would take about three seconds for the rock to reach this height and another three seconds for it to descend, a total of six seconds. Six seconds is one-tenth of a minute or 1/240th part of an hour. Since the earth has an equatorial circumference of about 24,000 miles, and rotates once in twenty-four hours, a point on the equator moves at a speed of 1,000 miles per hour. In these six seconds, or 1/240th part of an hour, a point on the equator would move about four miles. Hence, one would expect that during the time in which a rock was thrown straight up in the air for 150 feet and descended, the point directly underneath would have rotated, and the rock would strike the earth not at the point from which it was thrown vertically up, but rather at a point some four miles to the west. Since this does not happen, but since the rock does strike the earth at about the same point from which it was thrown up, either the Copernican system is absurd to the extent of contradicting experience, or the reasoning just presented is wrong.

One can see, therefore, why the acceptance of the Copernican system made necessary a wholly new system of physics. That this new system of physics, with its revolutionary implications for all of science, was achieved by Galileo and Newton is well known. But it is not equally well known just how this question was discussed from the time of Kepler and Galileo until it achieved its final solution by Newton. Professor Koyré, in this work, analyzes carefully the way in which this problem was treated by major and minor figures and thereby provides not only a background to the achievement of Newton, but also shows us the way in which thoughtful men of science struggle with major problems. That none of the individuals studied here was actually successful is not particularly important, since even Newton made a serious mistake in an early attempt to solve this problem. Thus Professor Koyré has given us a new index to the supreme genius of Newton, while also giving a new dimension to our understanding of the difficulties raised by the acceptance of the Copernican system.

ESMOND R. LONG. *A History of the Therapy of Tuberculosis and the Case of Frédéric Chopin.* Lawrence, Kansas, University of Kansas Press, 1956. 71 pp. $2.00.

Reviewed by ROBERT H. GREEN, *Associate Clinical Professor of Medicine, Yale University School of Medicine.*

THIS brief volume, the sixth series of the Logan Clendening Lectures on the History and Philosophy of Medicine, presents an interesting outline of the history of the therapy of tuberculosis. Probably few, if any, medical problems have given rise to more controversy or been associated with more emotional reaction on the part of patient and physician alike than the treatment of phthisis. Many of those who have contributed much to its study have themselves been plagued with tuberculosis. In this day of great achievement with effective antituberculous chemotherapy and excisional surgical treatment a review of past failures and accomplishments is all the more timely.

One is amazed at the multiplicity of therapeutic measures employed over the ages and at the conviction with which some, such as bleeding, purging, and the consumption of obnoxious concoctions, were advocated by eminent authorities. Benjamin Rush vigorously practised bloodletting to the extent of a pint or more a week, and Thomas Sydenham proclaimed horseback riding to be the "principal assistant in the cure of this disease." As better methods of diagnosis and greater knowledge of the pathological features of tuberculosis became available a more cautious note concerning the efficacy of therapy was sounded by some. Bayle and Laënnec, both consumptives and both outstanding phthisiologists, obviously believed that cavitary disease was incurable. In retrospect it appears that little progress was made in the struggle for effective treatment until the advent of sanitorium care which was introduced at about the time of the discovery of the

tubercle bacillus by Koch in 1882. Progress in controlling the spread of the disease soon followed but for years to come there often was little scientific basis for the therapy prescribed. Diametrically opposed forms of treatment, such as violent exercise as opposed to complete bed rest, were recommended by equally authoritative clinicians. Eventually the relatively successful regime of rest and selective collapse procedures were evolved and only now are being superseded by the remarkable advances of chemotherapy and resectional surgery of the past decade.

The account of Chopin's life not only illustrates the details of treatment prevalent during that period, but also incites curiosity as to the effect of tuberculosis on his achievements. A similar, but perhaps duller version, could be furnished of the lives of many other geniuses who have been afflicted with tuberculosis. In the case of Chopin the tale is a fascinating one. The ineffectiveness and miserable discomforts of the treatments of the day are obvious, and one is aware of the incessant coughing, hemoptysis, and other loathsome and often frightening features of far advanced phthisis. The three great influences in Chopin's life were his music, his loves, and his tuberculosis. That each had a profound influence upon the others seems certain, and one can only speculate whether the world might not be richer by many brilliant compositions had death not come at so early an age.

ARTHUR WILLIAM MEYER. *Human Generation; Conclusions of Burdach, Döllinger and von Baer.* Stanford, California, Stanford University Press; London, Geoffrey Cumberlege, Oxford University Press, 1956. xi, 143 pp.

Reviewed by THOMAS R. FORBES, *Associate Professor of Anatomy, Yale University School of Medicine.*

PROFESSOR Meyer presents translations of the "pronouncements on generation" of three nineteenth century German biologists, together with his own comments on the men and their work. He has chosen to translate rather literally and has met with skill the problem of selecting English technical terms which are equivalent but which do not imply concepts which arose only after the German originals were written. Pages 7 through 25 carry translations by graduate students in classics of two Latin monographs by Burdach. Almost two-thirds of the book is devoted to von Baer's *Commentar* and to a discussion of it and of the great German embryologist.

In themselves, the "pronouncements" often seemed to this reviewer rather tedious. The nineteenth century's imperfect understanding of reproduction and embryology could have been illustrated at less length. Again, if these writings had disclosed important contributions to the science of the period, they would be exciting, but, with a few exceptions, the passages which have been translated scarcely are likely to stir the average biological reader. On the other hand, this material will demand the interest of the specialist in the history of embryology.

Dr. Meyer's "Introduction" and chapters of comment are crowded with historical details, many of them interesting. Occasional discursions and frequent lack of organization are troublesome, as is a failure to identify some obscure individuals. For example, in a list (page vi) of contributors to the study of reproduction, there is included ". . . James Blundell, who in 1819 corrected Haighton's grievous error of 1797. . . ." But who was James Blundell, and who was Haighton, and what, indeed, was the latter's grievous error? We are not told. Parts of the sketch of von Baer's life are clear, vigorous, and absorbing, and the "Biographical Synopsis of von Baer" near the end of the volume is masterly.

There is a series of nine portraits of von Baer, Döllinger, Burdach, Cuvier, and von Liebig. Format and binding are pleasing. A list of references is included, but not an index.

RICARDO ARCHILA. *Bibliografia Medica Venezolana.* 2d Ed. Caracas, Bellas Artes C.A., 1955. 1,041 pp.

Reviewed by H. GARCÍA-AROCHA, *Associate Professor of Physiology, McGill University, Montreal, Canada.*

THE scope of this bibliographical research, written in Spanish, stretches over a century and covers papers published up to 1951. The author and subject indices, arranged in alphabetical order, aim to present the Venezuelan contributions in the field of medicine and cognate sciences. However, the publications listed in this volume are not restricted to medicine, or even to the applied biological sciences in general, but include subjects as alien to medicine as biographies, essays, chronicles, and speeches delivered in academies or at convocation ceremonies. It seems as though the purpose of the author has been to cover all the papers, scientific or otherwise, published by medical men, and to include some monographs written by nonmedical contributors with reference to medicine or physicians.

This volume is not merely an exhaustive collection of titles and authors but a many-sided demonstration of the *sui generis* type of literature which, until recently, flourished in some Latin American countries. Glancing at the pages of Archila's book one finds papers on the psychopathology of the Liberator, the conquest and colonization of the province of Caracas, psycholiterary studies on Zola's personality, the effect of heat on social evolution, and the influence of Darwinism on the progress of modern medicine. In addition, there are many eulogies delivered in academies, either on the occasion of welcoming a new member or in funeral orations to departed ones. Intermingled with these publications one finds serious contributions to modern medicine, discussions of clinical cases, basic research in malaria, descriptions of new surgical techniques, findings on the regulation of respiratory movements through lung chemoreceptors, and original investigations on serum inorganic phosphorus in its relation to carbohydrate metabolism.

This peculiar mixture of serious medical research and simple literary vein, in the references compiled by Archila, is plagued with contributions of certain men having wit, intelligence, and skill with words, but with no scientific training at all. Unfortunately, some of these papers carry with them tendencies which are inimical to fairness and encourage distortion of the scientific method.

I venture to say that it is only in the past twenty years that a more responsible approach to basic medical sciences has started in that Caribbean country. Relevant in this connection is the impulse given by distinguished scholars who have made their homes in Venezuela in the past two decades; among them Professors A. Pi-Suñer, M. Mayer, and R. Jaffe whose contributions to physiology, tropical medicine, and pathology, respectively, have encouraged the enthusiasm of the local scientists. In addition, more and more young doctors are now travelling abroad to acquire scientific or professional training in specialized centres of Europe and North America.

Dr. Archila's work in compiling these bibliographical references has been painstaking and meticulous. He has been confronted with the inherent difficulties of tracing papers which appeared in short-lived publications or in journals of limited circulation. His sources of information have been so diversified that the author has searched from daily newspapers to academic bulletins. Dr. Archila deserves praise for having written this volume, the first attempt ever made in Venezuela toward a comprehensive review of the vernacular medical literature.

A. J. Berry. *From Classical to Modern Chemistry. Some Historical Sketches.* Cambridge, Cambridge University Press, 1954. xii, 251 pp. $4.75.

Reviewed by Harold G. Cassidy, *Associate Professor of Chemistry, Yale University.*

The author says that his primary purpose in this volume is to consider "the historical development of certain branches of chemical science, which were either omitted from, or only adverted to in brief outline, in an earlier book, *Modern Chemistry—Some Sketches of its Historical Development,* published in 1946." This he has done in the form of nine compact and fact-filled essays. Each essay takes up one particular topic: Theory in Chemistry, Analytical Chemistry, Chemical Formulae, Kinetic Chemistry, among others, and develops it along the thread of time, intentionally giving particular emphasis to older parts of chemistry and continually showing the interplay between chemical and physical developments. In this way the author has achieved an integration that is attractive and enlightening to the reader. His writing gives a feeling of pace.

One discovers anew in reading this book that many inventions, experiments, and discoveries named for individual chemists or physicists had roots going much further back. But there is no feeling of hair-splitting or ex-

cessive preoccupation with priorities. Rather, the author leaves one with a sense of the various contributions that came together to make a discovery or a theory. For example, in the essay on "Some Aspects of Classical Electricity and Electrolysis," he states: "In 1843 Wheatstone devised his well-known bridge method for comparing resistances, which he had derived from an idea due to Christie some years previously. . . ." There are, throughout the book, all sorts of hints of work that one would like to look into—such as Kelvin's dropping water electrode (1860). To assist in further reading, each esasy is followed by a list of references.

Four other essays deal with the theory of heat, physical optics and chemistry, molecular magnitudes, and problems relating to valency. The essay on valency is interesting because it places Werner's ideas in the context of the times, and makes an often forgotten point with a quotation from Ephraim to the effect that valence is a number, and not a force. Each essay contributes information that gives the reader a sense of the powerful cooperative advance of the physical sciences. This is a book that students as well as professional physicists and chemists should enjoy and refer to often. It is nicely printed.

W. R. FEASBY, Ed. *Official History of the Canadian Medical Services 1939-1945*. Vol. I. Ottawa, Queen's Printer, 1956. xii, 568 pp. $5.00.

Reviewed by WILLIAM C. GIBSON, *Kinsmen Professor of Neurological Research, University of British Columbia, Victoria, Canada.*

COLONEL Feasby and his colleagues, J. P. McCabe, J. C. Morrison, W. A. McKay, and J. P. McLaughlin have concentrated in Volume I of the official story of the medical services of Canada's armed forces an amazing amount of detail of six long years of war. It is sobering reading at times. It is not the flamboyant chronicle of a single campaigner, a political has-been, or a self-styled messiah. It is, rather, a mosaic of the far-flung Canadian medical effort, military and civil—including Navy, Army, and Air Force, the Red Cross, the St. John Ambulance, and the Federal Department of Health and Welfare.

At the outbreak of World War II in 1939 Canada's armed forces boasted only 40 permanent medical officers, all Army. The Navy had been without a medical officer for fifteen years! The Air Force had never had a medical service. An amazing transformation took place as Canada's war effort developed. It was possible to lend the Royal Navy in the United Kingdom one hundred medical officers. Gradually Canadian doctors came to care for the large numbers in Canadian units overseas. This change, after several years of war, produced widely sensed repercussions, as when Royal Canadian Air Force psychiatrists sought to modify the "Lack of Moral Fibre" nihilism, as some felt it to be, of the Royal Air Force.

This volume is devoted to "Organization and Campaigns," so that the reader may feel that a disproportionate emphasis has been placed on estab-

lishments as against the purpose which they were intended to serve. It should be made clear that Volume II is concerned with the clinical and professional interest of the war. A word should be said on the geographical maps by Capt. C. C. J. Bond, from which the campaigns can be followed. They are plentiful, clear, and instructive. The statistical information concerning the British Commonwealth Air Training Plan is a first-class demonstration of the value of a well-conceived statistical department in charting the progress of a rapidly expanding service.

Whether this volume, an "Official History," contains the "relevant history" of the armed forces medical services or not, only time can tell. It was written within ten years of the conclusion of hostilities. Twenty years from now perspectives may have changed considerably. However, between the lines of Colonel Feasby's treatise one senses the one element of war which never seems to change, the decerebrate rigidity, not to mention the upgoing eyebrow associated, so often, with higher level decisions. The delays which frustrated so many active men at the front, so many designers of equipment, and so many researchers, are passed over lightly by Colonel Feasby, though they could teach us a great deal about the approach required in planning for future emergencies. However, this is not a new problem. In 1859 Florence Nightingale wrote in her remarkable *Sanitary History of the British Army during the late war with Russia:*

> The three things which all but destroyed the army in the Crimea were ignorance, incapacity and useless rules; and the same thing will happen again, unless future regulations are framed more intelligently, and administered by better informed and more capable officers.

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